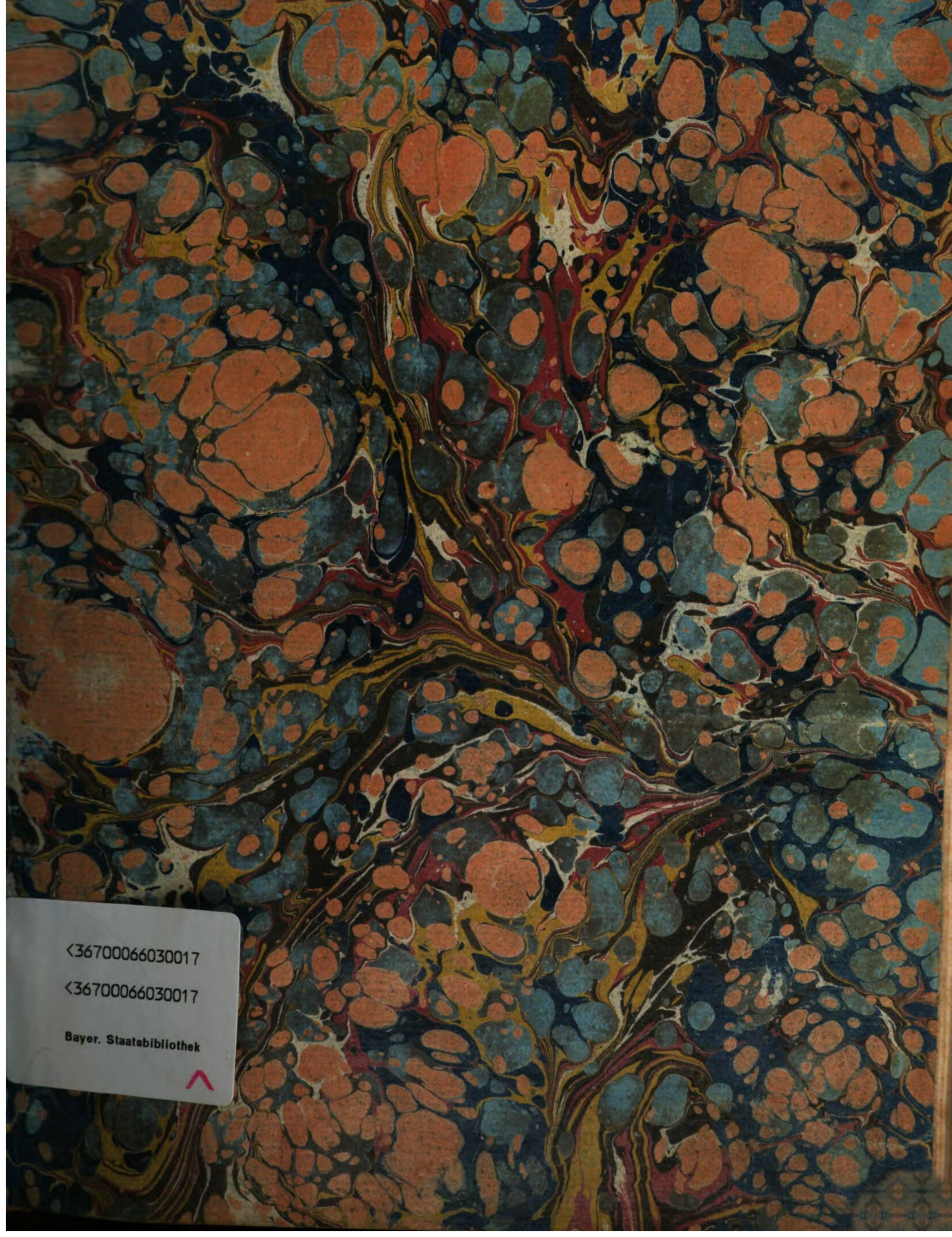








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T H E
W O R K S
O F

ROBERT WHYTT, M. D.

Late PHYSICIAN to his MAJESTY;
PRESIDENT of the Royal College of PHYSICIANS,
Professior of Medicine in the Univerfity of EDINBURGH,
and Fellow of the ROYAL SOCIETY.

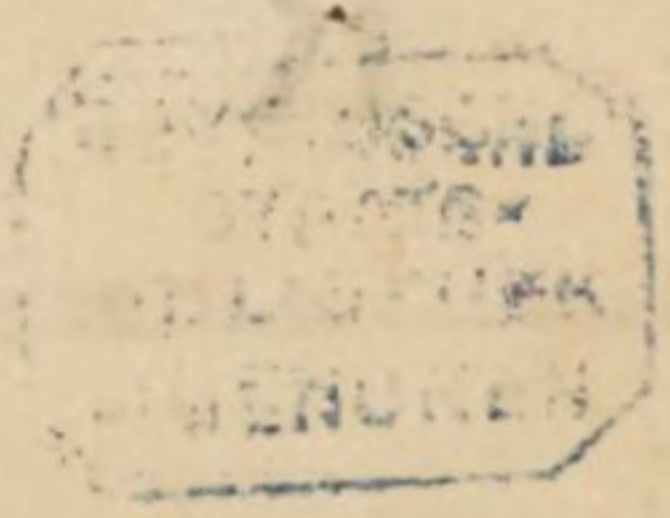
Published by his SON.

EDINBURGH:

Printed for T. BECKET, and P. A. DEHONDT, London;
and J. BALFOUR, Edinburgh.

By BALFOUR, AULD, and SMELLIE,

M.DCC.LXVIII.



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By BALFOUR, ARD, and SMELLIE,

MDCCLXXIII.



T O

Sir J O H N P R I N G L E, Baronet,

PHYSICIAN to her MAJESTY;

In Testimony of the sincere Friendship that subsisted between
him and the A U T H O R ;

A N D

In Gratitude for the Care he has taken of this complete E D I -
T I O N of his W O R K S,

They are most respectfully dedicated

by his S O N,

R O B E R T W H Y T T.

T O

SIR JOHN PRINGLE BRIDGES

PHYSICIAN TO HER MAJESTY

In Testimony of the sincere friendship that subsisted between
him and the Author;

A N D

In Gratitude for the Care he has taken of this complete Edition of his WORKS,

They are most respectfully dedicated

by his SON,

ROBERT WHITT.

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contained in this COLLECTION.

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A N
E S S A Y

ON THE
VITAL and other INVOLUNTARY
MOTIONS of ANIMALS.

Inanimum est omne quod pulsu agitur externo; quod autem est animal, id motu ciatur interiore et suo. Nam hæc est propria natura animi atque vis.—Quæ sit illa vis, et unde sit intelligendum puto. Non est certe nec cordis, nec sanguinis, nec cerebri, nec atomorum.

CICERO, Disput. Tuscul. lib. I.

The first Edition printed in the year 1751, and the 2d in the year 1763.

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Non est certe nec cordis, nec sanguinis, nec cerebri, nec stomachi.
Cicero, *De Partibus Animalium*, lib. 1.

The first Edition printed in the year 1751, and the 2d in the year 1763.

TO THE RIGHT HONOURABLE
JAMES EARL OF MORTON,
LORD ABERDOUR, &c. &c. &c.

IT is not your Lordship's high station in the world, but your extensive knowledge of the works of nature, and taste for Philosophical inquiries, which have determined me to inscribe the following Essay to your Lordship, and makes me, with pleasure, embrace this opportunity of publicly declaring the great respect with which I am,

My LORD,

Your LORDSHIP'S

most obedient, and

most humble Servant,

ROBERT WHYTT.

TO THE RIGHT HONOURABLE

JAMES EARL OF MORTON

LORD ABERDORN & CO.

I am not your Lordship's High Lord in the world, but your
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with which I am,

My Lord,

Your Lordship's

most obedient and

most faithful servant,

ROBERT WHITE.

PREFACE.

ABOUT twelve years ago, not long after the author of this essay had left the schools of medicine, he began to be dissatisfied with the common theories of respiration and the heart's motion; and as he had not met with any writer, who had given, as he thought, a just account of the vital and other involuntary motions of animals, or derived them rightly from their true source, he purposed some time or other to write on this subject, if not for the public, at least for his own satisfaction. In pursuance of this resolution, the following essay was begun in the year 1744, and might have been finished long ago, had not the author's time been greatly taken up with more necessary business. In composing it, he has been careful not to indulge his fancy, in wantonly framing *hypotheses*, but has rather endeavoured to proceed upon the surer foundations of experiment and observation. No doctrine in philosophy, which was not built on these, has ever been able to stand its ground for half a century; and the theories of Newton, and some few others of the more happy philosophers, have therefore triumphed over all objections, because they were founded on nothing else but plain facts; facts indeed, whose existence was perhaps unknown before, and whose influence is so extensive, that while they are simple and uniform in themselves, they serve as causes for explaining innumerable effects. On the other hand, in the hypothetical method of philosophizing, causes are usually assigned, which not only cannot be proved to exist, but which are frequently more intricate and complex than even the effects to be explained from them. And indeed, it cannot be expected that unguided imagination

imagination should hit upon the truth, since nature has so closely concealed many of her operations, that they often elude the united efforts of genius, industry, and experiment.

THERE is one favour which the author would ask of those who may take the trouble to peruse this performance, *viz.* that they would delay passing judgment upon any part of it, till they have attentively and fairly considered the whole; because it is apprehended, that the theory of every one of the motions here explained, supports and strengthens what is said of the rest, and that, when all are taken together, each receives an additional weight of argument, and appears in a stronger light.

EDINBURGH,
OCTOB. 1. 1751.

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O F T H E
V I T A L and other I N V O L U N T A R Y
M O T I O N S of A N I M A L S.

I N T R O D U C T I O N.

PHYSIOLOGICAL writers have divided the motions of animals into voluntary, involuntary, and mix'd.

THE voluntary motions are such as proceed from an immediate exertion of the active power of the will. The involuntary and mix'd motions (which last, though subject to the power of the will, yet are not ordinarily directed by it) may be aptly enough comprehended under the general denomination of SPONTANEOUS; since they are performed by the several organs as it were of their own accord, and without any attention of the mind, or consciousness of an exertion of its power: such are the motions of the heart, organs of respiration, stomach, intestines, &c.; which have been also distinguished by the term AUTOMATIC; though perhaps there is an impropriety in the word, as it may seem to convey the idea of a mere inanimate machine, producing such motions purely by virtue of its mechanical construction: a notion of the animal frame, which ill agrees with the *inertia* and other known properties of matter.

ALTHOUGH we may be at a loss to explain the nature of that substance in the nerves, by whose intervention the mind seems to act upon the muscles; and though we may be unacquainted with the subtile structure of those fibres upon which this substance operates,

rates, yet we have no reason to doubt that voluntary motion is produced by the immediate energy and agency of the mind; manifold experience convincing us, that though there be required certain conditions in the body in order to its performance, it is nevertheless owing to the will. Nor ought we to be surpris'd when we meet with these kind of difficulties; for they attend most of our inquiries and researches: Thus, though the laws of motion and gravitation be fully understood and demonstrated by philosophers; yet the first cause of motion, the manner in which it is communicated to bodies, and the nature of gravity itself, have never been explained.

BUT how it comes to pass that many of our muscles are brought into contraction, not only without the concurrence of the will, but in opposition to its strongest efforts, and why most of the organs of spontaneous motion are continually agitated with alternate contractions and relaxations, of which we are not conscious, while the muscles of voluntary motion remain at rest, and are not contracted but in consequence of a determination of the will to that end; are questions which have occasioned no small debate among medical writers, and about which as yet they are far from being agreed. To clear up these points, is the principal design of this Essay; and I flatter myself, that the following account of the vital and other involuntary motions of animals, will not less recommend itself to equal judges by its simplicity, than by its agreeableness to the known laws of the animal œconomy, and the easy solution it affords of all the appearances of the human frame with respect to its involuntary motions.

NATURE, as far as we can judge from the plan and scheme of things surrounding us, delights in simplicity and uniformity, and, by general laws applied to particular bodies, produces a vast variety of operations; nor is it improbable that an animal body is a system regulated after the same manner. Following the path, therefore, which Nature has pointed out in her other and more grand operations, I have in this essay endeavoured to shew, that all the spontaneous motions of animals are explicable upon the same principle,
and

and owing to one general cause. How far some authors of great note have been unsuccessful in their inquiries into this matter, from their neglecting so obvious an analogy, and endeavouring to explain the vital motions of almost every different organ, by a different theory, is left to the reader to judge.

As the heart is one of the principal organs of the body, and its action immediately necessary to life, I shall begin with inquiring into the cause of its alternate contraction and relaxation, and whence it happens that these motions are performed without the mind's seeming to have any concern in them, nay in opposition to the strongest efforts of the will. But it will be necessary, previously, to lay down a few *postulata*, as a ground-work upon which to build our theory of the involuntary motions of animals in general, and of that of the heart in particular.

S E C T. I.

Principles and facts necessary to be premised.

I. **A** CERTAIN power or influence lodged in the brain, spinal marrow, and nerves, is either the immediate cause of the contraction of the muscles of animals, or at least necessary to it.

The truth of this appears from the convulsive motions and palsies affecting the muscles, when the *medulla cerebri*, *medulla oblongata* and *spinalis*, are pricked, or any other ways irritated or compressed; as well as from observing that animals lose the power of moving their muscles, as soon as the nerve or nerves belonging to them are strongly compressed, cut through, or otherwise destroyed. Of this many instances might be given: But we shall content ourselves with mentioning one. When the recurrent nerve on one side of the *larynx* is cut, the voice becomes sensibly weaker; when both are cut, it is entirely and irrecoverably lost *, *i. e.* the animal loses the power

A 2

* Edinburgh Medical Essays, vol. 2. art. 8.

power of moving the muscles which serve to increase or diminish the aperture of the *glottis*; for I presume it will be now needless to shew that the tying of those nerves can only affect the voice, by rendering these muscles paralytic *.

If the brain were not in a manner the fountain of sensation and motion, and more peculiarly the seat of the mind than the other *viscera*, or members of the body; why should a slight inflammation of its membranes cause madness, or a small compression of it produce a palsy or apoplexy, while a like inflammation of the stomach or liver, or a compression or obstruction of these bowels, have no such effects? If the nerves were not immediately concerned in muscular motion, why, upon tying or destroying them, does the member to which they are distributed, lose all power of motion as well

* In the *Comment. Acad. Bonon. vol. 2. part 2.* are related by Molinelli, the cases of two patients, who, though they had, in the operation for the aneurism in the arm, the nerve tied with the artery, yet recovered, after about three months, the entire use of that member; whence some have concluded, that the nerves are not necessary to motion or sensation. But Galen informs us, that as often as a nerve has been quite cut through, the muscles to which it belonged were deprived both of sense and motion†: and many later examples might be produced, where the same consequence attended the destroying of a nerve. I shall only mention one, which consists with my own knowledge. J. F. who had the nerve tied with the artery in the operation for the aneurism eighteen years ago, continues to have a numbness and feebleness of the muscles of the thumb and forefinger, which are also a good deal shrivel'd. But further, it appears, even from the cases now mentioned, that the immediate consequence of a ligature made upon the nerves was a total loss of motion and sensation in the parts below; and this happened notwithstanding that the blood continued, by two pretty large arterial branches, to be distributed to them; which is such a direct proof of the necessity of the nerves in order for having motion and sense, as is not to be overturned by that circumstance of the parts recovering afterwards their power of motion, since this might happen without any inconsistency to the former conclusion, and in a way unknown to us. In the case found by Morgagni among Valsalva's papers, and related in the same volume of the *Comment. Bonon.* we are told, that the patient did not recover the full use of his arm till eight or nine months after the operation for the aneurism was performed. When Molinelli dissected this arm thirty years after, he found the nerve not wanting in the place where the ligature had been made, as were the artery and vein, but of a much greater thickness than usual, and not unlike a *ganglion*. From this observation, I think we have reason to believe, that, in Molinelli's two patients above mentioned, the nerve was not destroyed by the ligature, but had acquired a greater thickness in that part, and so became, after some months, fit to perform its functions.

Upon the whole, the cases of the operation for the aneurism related in the *Bolonian Transactions*, though they may perhaps raise some doubts, will never incline any attentive inquirer to reject the received doctrine, of the nerves being necessary to motion and sensation.

† De motu musculorum, lib. 1. cap. 1.

well as sensation? Because animals have lived with a brain so diseased, that it is difficult to conceive how it could perform its functions, or because monsters which have been born without a head, lived some short time, and had the power of motion; to conclude, I say, from hence, that the brain and nerves in perfect animals are not immediately necessary to motion and sensation, is as if we were to assert, that the heart was not designed to propel the blood through the body, because there are several animals of the lowest class which have no such organ *, and monstrous foetuses have sometimes wanted it †; or because we are told of a rat seemingly healthful, though upon dissection it was found to have no heart ‡.

THE reasoning drawn from a few monstrous cases, is not sufficient to overthrow a doctrine founded upon the plainest appearances in perfect animals, and confirmed by numberless experiments made upon them. The influence therefore of the brain and nerves towards producing muscular motion, is not to be disproved by a few rare instances of ossified, petrified, or otherwise morbid brains found in animals which seemed tolerably healthy, and had the motion of all their muscles; since it is not more unreasonable to suppose, that the nerves may derive nourishment from a porous spongy ossified brain, than that a tree should spring out of a stone-wall; dry stone and lime being not less different from moist earth, than such an ossified brain from one in its natural state ||. When the brain is wanting, Nature may have other ways of supplying the nerves, and of keeping them in such order, as that they may be able in some sort to perform their functions. And since the spinal marrow not only depends, for its powers, on the brain from which it proceeds, but also on a particular secretion performed by its own blood-vessels, we may reasonably conclude, that the
nerves

* Harvey de motu sang. cap. 17.

† Memoires de l'Acad. des sciences 1720. Edit. 8vo. p. 16.

‡ Van Swieten Comment. in Boerhaav. aphorism. vol. 1. p. 256.

|| The brain mentioned by Duverney, in Memoires de l'Acad. des sciences 1703, edit. 8vo, p. 318. &c. was not wholly petrified; for its inferior part, from which the nerves take their rise, still retained its medullary form. And the same has probably been the case of other petrified or ossified brains, though perhaps not so accurately inspected.

nerves proceeding from the brain and spinal marrow are partly nourished and kept in a proper state for action, by the fluids conveyed to them, by the small arteries which are distributed on their surrounding membranes. This opinion is supported by those cases of children which have been born without any brain or spinal marrow; for in them the nerves must have derived all their nourishment from the blood-vessels bestowed on their coats.

THE immediate cause of muscular contraction, which, from what has been said, appears to be lodged in the brain and nerves, I chuse to distinguish by the terms of the *power* or *influence of the nerves*; and if, in compliance with custom, I shall at any time give it the name of *animal* or *vital spirits*, I desire it may be understood to be without any view of ascertaining its particular nature or manner of acting; it being sufficient for my purpose, that the existence of such a power is granted in general, though its peculiar nature and properties be unknown.

2. WHILE the nervous power is immediately necessary to muscular motion, the arterial blood seems to act only in a secondary or more remote manner.

MUSCLES instantly become paralytic upon tying or destroying the nerves distributed to them *. But when the arteries bestowed upon any muscles are tied, the action of those muscles is only gradually weakened, and not totally abolished till after a considerable time. Dr Langrish tied up and cut asunder both the carotid and both the crural arteries of the same dog, without destroying the motion of one muscle †; and Dr Schwencke assures us, that, after having tied the crural artery of a dog close by the groin, the animal continued to move his leg and foot for a whole day; the same experiment he repeated in another animal, and did not find that the muscles of the leg became paralytic till this member was almost quite dead ‡. 'Tis true indeed, that, by a li-
gature

* Kaau Impet. faciens, No. 288.

† Cronean Lectures on muscular motion, § 93.

‡ Hæmatalog. p. 8. See also Brunner. de pancreat. p. 188.

gature made on the *aorta*, immediately above its division into the iliacs, the hinder limbs of a dog gradually lost their motion, and became quite paralytic after two minutes*: from which it may be thought, that, in the experiments of Langrish and Schwencke, the motion of the muscles continued longer, because they had still some blood transmitted to them by lateral communicating branches, from arteries which were not tied. But, on the other hand, it seems more probable, that a ligature on the *aorta* renders the muscles of the legs paralytic, by depriving the inferior part of the spinal marrow, and the nerves proceeding from it, of that blood which contributes to nourish and keep them in a sound state [1.].

FROM what has been said, we may fairly conclude, that the arterial blood sent to the muscles is only necessary to their motion, in so far as it supplies the vessels and fibres of the muscles with fluids proper for their nourishment, gives them a suitable degree of warmth, and thus preserves them in such a state as may render them most fit to be acted upon by the nervous power. While therefore the nourishment and growth of the muscles are owing to the motion of the arterial blood through their vessels, their powers of motion and sensation proceed from the nerves alone.

3. THE muscles of living animals are constantly endeavouring to shorten or contract themselves. Hence such as have antagonists are always in a state of tension; and the solitary muscles, such as the sphincters, and those whose antagonists are weakened or destroyed, are always contracted, except when this natural contraction is overcome by some superior power.

4. THE natural contraction of the muscles [3.] is owing partly to all their vessels being distended with fluids, which separate and stretch their smallest fibres.

As a proof of this, the muscles of animals that are in full health, and abound with proper fluids, contract themselves more remarkably towards each extremity when cut across, than the muscles of
such

* Kaau Impet. faciens, No. 291.

such animals as are in a languishing state, and exhausted of their fluids; besides, that soon after death, the muscles become flaccid, and, when cut transversely, contract themselves but little.

BUT, farther, the natural contraction of the muscles is, in a great measure, to be ascribed to the influence of the nerves, which is perpetually operating upon them, though in a very gentle manner: and that to this is chiefly owing the constant contraction of the sphincters, and the tension of such muscles as are balanced by antagonists, the palsy affecting the sphincters as soon as their nerves are compressed or destroyed, and the constant contraction of such muscles whose antagonists are deprived of the nervous power, evidently demonstrate.

5. THE natural contraction of the muscles [3. and 4.] arising from the constant and equable action of the nervous power on their fibres, and of the distending fluids on their vessels, is very gentle, and without any such remarkable hardness or swelling of their bellies, as happens in muscles which are contracted by an effort of the will. And although the sphincters and those muscles whose antagonists are paralytic or hindered from acting, do always remain in a state of contraction; yet at any time, by an effort of the will, they can be much more strongly contracted. Dr Stuart was therefore under some misapprehension when he said, that the mind has no manner of power over such muscles as are destitute of antagonists; not only that it cannot unbend them, which is allowed by all, but also that it cannot make them contract more strongly*; for every one must be satisfied, that though the *sphincter ani* is naturally in a constant state of contraction, yet he can at pleasure make it contract more strongly; and though the *biceps flexor cubiti* contracts and swells upon the arm's being bent by an external force, even in spite of any effort of the will to the contrary; yet any one, if he pleases, can make it swell more, grow much harder, and contract itself with vastly greater force.

FROM what has been just now advanced, it follows, that it is not necessary, in order to the mind's acting upon the muscles, that they

* Dissertatio de motu musculari, p. 22. 23. and 77.

they should be stretched or extended beyond that length to which they would naturally reduce themselves, if not prevented by the action of their antagonists.

6. As often as the influence of the nerves operates more powerfully than usual on the muscles, they are excited into stronger contractions, which are not natural, and therefore may be called violent. This extraordinary action of the nervous influence may be owing either to the power of the will, or to a *stimulus*.

7. VOLUNTARY contraction is owing to the stronger action of the nervous influence upon any muscle, excited by the power of the will.

8. A *stimulus*, or any irritating substance applied to the bare muscles of living animals, immediately produces a contraction in them.

THIS appears from numberless experiments and observations; and is equally true with respect to the muscles of voluntary and involuntary motion. The muscles of a living frog, when laid bare and pricked with a needle, are strongly convulsed. A solution of white vitriol no sooner touches the internal surface of the stomach, than this organ is brought into convulsive contractions. Smoke of tobacco or acrid clysters injected by the *anus*, bring convulsive motions on the great guts. Pricking the intestines or heart of a living animal, or applying any acrid fluid to them, remarkably increases their contraction*. Many other instances might be given of the effects of *stimuli* on the muscles of animals; but these may suffice, as we shall have occasion to treat of this matter more fully afterwards.

WHATEVER stretches the fibres of any muscle, so as to extend
B them

* Harvey, treating of the *punctum saliens*, or heart of the chick in the shell, says, "Vidi
" sæpissime ab acus, styli, aut digiti contactu, imo vero a calore aut frigore vehementiore ad-
" moto, aut cujuslibet rei molestantis occurſu, punctum hoc pulſuum varias permutaciones
" iſuſque validiores ac frequentiores edidiſſe." De generatione animal. exercitat. 17.

them beyond their usual length, excites them into contraction almost in the same manner, as if they had been irritated by any sharp instrument, or acrid liquor. Thus the motion of the heart in pigeons newly dead, is as sensibly renewed or increased by drawing asunder the sides of the divided *thorax*, and consequently stretching the great vessels to which the heart is attached, as by pricking its fibres with a pin *. In luxations, muscles, by being over-stretched, are often convulsed; and the *vesica urinaria* and *intestinum rectum* are not only excited into convulsive contractions by the acrimony of the urine and *fæces*, but also by their bulk and weight stretching the fibres of these hollow muscles †.

BECAUSE the heart and other muscles of animals, often, continue to move, for some time, after they are separated from their bodies, and consequently after all communication between them and the brain is cut off, some have thought the contraction of irritated muscles to be owing, not so much to the nervous influence, as to some latent property in their fibres. But, as I have elsewhere given reasons for dissenting from this opinion ‡, I shall only observe, in this place, that unless the motions of irritated muscles depended upon the brain and nerves, it would be difficult to conceive why an irritation of the *medulla oblongata* or nerves should occasion more violent convulsions of the muscles in animals newly killed, than an irritation of those muscles themselves.

9. IN proportion as the *stimulus* is more or less gentle, so (*cæteris paribus*) is the contraction of the muscle to which it is applied.

THE truth of this proposition, like the former, is not only proved by experience, but may be deduced from reason alone; for if the irritation is to be considered as the cause, and the subsequent contraction of the muscle as the effect; then, in proportion as the cause is increased or diminished, so must be its effect. The motions occasioned by stretching the fibres of any muscle will be
greater

* Vid. *infra*, Sect. xiv. No. 16 and 17.

† Vid. *infra*, Sect. v.

‡ Physiological Essays, edit. 2. appendix, p. 245.—252.

greater or less, as the muscle is more or less stretched; unless it be so far extended as quite to lose its tone, and become paralytic. It ought however to be observed, that the effects of different *stimuli* depend very much upon the peculiar constitution of the nerves and fibres of the muscles to which they are applied: And hence it is, that what proves a strong *stimulus* to the nerves of one part, will more weakly affect those of another, and *vice versa*. Thus warm water or oil, which, when drunk in a large quantity, provoke vomiting, lessen the increased alternate motion of the small vessels of a part which has been inflamed by the application of *cantharides*, or any other acrid substance. Cold water, which is very agreeable to the nerves of the stomach, excites violent coughing as often as it gets into the windpipe. Light, which by irritating the *retina* occasions the contraction of the pupil, does not act, sensibly, as a *stimulus* on any other part of the body. Although an injection prepared with corrosive mercury occasions considerable pain, when it is introduced into the *urethra*, yet neither it, nor the urine excites any convulsive contractions of the *musculi acceleratores urinæ*, as the *semen* does, although it is of a soft, unctuous nature, and possessed of very little acrimony. And this leads me to observe, that the motions excited in our muscles are often not so much the consequence of pain as of a different kind of sensation: thus, when the sides, or the soles of the feet are tickled, convulsive motions are excited in the muscles of the trunk of the body and legs, which, however, do not happen when those parts are pricked with a pin, or inflamed by the application of a blister or sinapism. Further, the same organs in different people are sometimes very differently affected by the same *stimuli*; thus honey excites vomiting and purging in some people; and in others, certain *effluvia* will occasion an asthmatic fit, or hysteric faintings with convulsions. I shall only add, that very hot or corrosive substances, often excite less motion in the muscles, than milder *stimuli*, because they either at once destroy, or greatly impair the moving power of the parts to which they are applied; and hence it is, that the heart of a frog ceases,

almost immediately to move, when it is immerfed in boiling water or oil of vitriol.

10. AN irritated muscle does not remain in a contracted state, although the stimulating cause continues to act upon it; but is alternately contracted and relaxed.

THUS the *stimulus* of an emetic received into the stomach does not occasion a continued contraction of its muscular coat; and an irritation of the lower extremity of the gullet is followed by alternate convulsions of the diaphragm. The heart of a frog or eel taken out of the body continues its alternate motions while a needle is fixed in it. When the heart, or other muscular parts of dying animals, cease to move, heat will renew their contraction, which is regularly alternate, although the *stimulus* be unvaried: After the auricle of a pigeon's heart had ceased to move, I made it renew its alternate contractions, by filling the *thorax* with warm water*; and after the vibrations of the heart of a frog had begun to languish, they recovered their former vigour and quickness, when the heart was exposed to the heat of a fire.

WHEN muscles have been long in action, or too highly strained, the member to which they belong is observed to be affected with a *tremor*, which often lasts for a considerable time; *i. e.* these muscles are agitated with small alternate contractions and relaxations, notwithstanding the stimulating cause continues present with them.

IT might perhaps be imagined, that a muscle ought to remain contracted as long as the *stimulus* or cause of its contraction continues to act upon it: but the fact we see is otherwise; and the reason of it shall be explained afterwards†. But it must be here observed, that although there are scarce any of the muscles of animals whose fibres are not affected with alternate contractions and relaxations, when they are irritated with the point of a pin, or other substance capable of stimulating them, yet, in morbid cases, our muscles or muscular organs are frequently affected with a continued

* See §. xiv. No. 15. of this Essay.

† See below, Sect. x. near the end.

tinued contraction or fixed spasm; which symptom, however, is owing either to an irritation of the brain or nerves, or of the muscles themselves: nay, even in a sound state, there are a few instances of muscles which are not alternately relaxed, but remain uniformly contracted as long as the stimulating cause continues to act with the same degree of force, such as the orbicular muscle of the *uvea*, the bladder of urine, and some others *.

II. IRRITATED muscles are not only agitated with motions while the stimulating cause continues to act upon them, but also for some time after it is removed; although these motions become gradually weaker, and are repeated more slowly. If the irritation be great, these alternate motions last longer, and follow one another more quickly; if weaker, they are repeated after longer intervals, and sooner cease; if extremely gentle, and the muscle not very sensible, perhaps only a single contraction or two will ensue [9.]

SOME of the fibres of the *platysma myoides* which were dissected off with a tumor, have been observed to palpitate like the heart of a dying animal for some time; and the same motions have been often observed in the muscles of brute animals, when their fibres were irritated after their separation from the body †.

THE heart of an animal newly killed is excited into motion by blowing upon it, or touching it with the point of a pin; and this motion often lasts a great while, although the *stimulus* is not renewed. After the heart of a pigeon had ceased to move, its vibrations were not only renewed by drawing asunder the sides of the divided *thorax*, but they continued for a considerable time ‡.

12. THE motions of muscles from a *stimulus* are altogether involuntary.

EVERY one must be sensible of the truth of this assertion, who has ever felt any of those small convulsions, or pulsatory contractions, which frequently happen in different parts of the body, and which seem

to

* See below Sect. x. near the end.

† Vd. Sect. xiv. No. 3.

‡ Sect. xiv. No. 16. and 17.

to be owing to some irritation of the fibres or membranes of the muscle contracted, either from acrid particles in the fluids irritating their sensible nerves, or from too great a distension of their tender vessels by the stagnation of the circulating fluids. The muscles called *acceleratores urinæ*, though at other times entirely under the power of the will, yet, while the *semen* continues to be poured into the beginning of the *urethra*, they are agitated with strong convulsive contractions, which we can neither increase nor prevent. When the tendinous fibres of the *obliquus inferior* of the eye, or of any other of its muscles, are gently stimulated with the point of a file, the alternate contractions which ensue are altogether involuntary, and can neither be accelerated, retarded, augmented, nor diminished by the power of the will. The same thing is true of the motions of the stomach and diaphragm, excited by emetics. From which it follows, that,

13. THE power of *stimuli* in exciting the muscles of living animals into contraction, is greater than any effort of the will.

THE truth of this is still further confirmed by the following observation. A man aged 25, who, from a palsy of twelve years continuance, had lost all power of motion in his left arm, after trying other remedies in vain, at last had recourse to electricity; by every shock of which the muscles of this arm were made to contract; and the member itself, which was very much withered, after having been electrified for some weeks, became sensibly plumper. If then, the voluntary muscles can, even in a palsied state, be excited into contraction by the action of a *stimulus* on their fibres, it follows, that when this is applied to them in a sound and more sensible state, any effort of the will to prevent their contraction must be vain and impotent. Hence the muscles of voluntary as well as of involuntary motion cease to be under the power of the will, while their nerves or sensible fibres are irritated by *stimuli*.

IT may be observed here, that although we cannot, by an effort of the will, prevent the motion of any muscle whose fibres or nerves themselves are irritated, yet we can, in many cases, restrain the
action

action of certain muscles, whose motions are excited by an irritation of a distant part, with which they have a particular sympathy; thus, we can prevent the motions of the muscles employed in coughing, and in voiding the urine and *fæces*, when the *trachea*, bladder, and *rectum*, are only slightly stimulated; unless these parts have been rendered much more sensible than usual, by being inflamed, or deprived of their *mucus*.

14. THERE are three kinds of contraction observable in the muscles of animals, all of them different from each other, *viz.* natural [4. and 5.], voluntary [7.], and involuntary, from *stimuli* [8. 9. 10. 11. 12. 13.]. The first is gentle, equable, and continued, and is owing to the causes mentioned N^o. 4. The second proceeds immediately from the power of the will, is always stronger than the former, and may be continued for a longer or shorter time, or performed with more or less force, as one pleases. The third is strong, but, suddenly followed by a relaxation, seems to be a necessary consequence of the action of the *stimulus* upon the muscle, and cannot be affected, either as to its force or continuance, by the power of the will.

THAT continued contraction or fixed spasm, with which our muscles are sometimes affected [10.] being almost always a morbid symptom, and not the natural or usual effect of an irritation of their fibres, its particular consideration is of less use in treating of the vital and other involuntary motions of animals in a sound state.

15. THE natural contraction above explained [14.] is what we observe in the sphincters, and in muscles whose antagonists are paralytic or destroyed.

16. WHILE the sphincters of the *anus* and bladder, and those muscles whose antagonists are destroyed, remain always in a state of contraction, and while such muscles as have antagonists are kept in *equilibrio*, or without any motion, except when the will interposes; the heart, which has no proper antagonist, is alternately

ly contracted and dilated, without our being able, by any effort of the will, directly to hinder or promote its motions.

17. THE contraction of the heart is, therefore, not only involuntary, but of a different kind from that of the sphincters and muscles deprived of antagonists; and seems, as to its appearances, to agree with the contraction of muscles from a *stimulus* [14.].

18. THE mind may, by disuse, not only lose its power of moving even the voluntary muscles, except in a particular way, but also of exciting them into contraction at all. Of the former we have an example in the uniform motions of the eyes; and of the latter in the muscles of the external ear, and of such members as have remained long without motion.

S E C T. II.

An examination of the opinions of some of the most considerable authors concerning the motion of the heart.

IT seems to have been the prevailing opinion among many of the ancient physicians, that the motion of the heart was owing to a vital principle particularly residing in it. Galen thought motion as natural to the heart, as rest to the other muscles. Des Cartes, much less versed in physiology than in mathematics, attributed the motions of this organ, wholly, to the ebullition of the blood dropping into its ventricles; and contended, that this fluid was not pushed into the arteries by the muscular contraction of the heart, but that it forced its way into them by its own explosive power. After Harvey's doctrine of the circulation was fully established, the heart was allowed to be a muscle, and its *systole* to be analogous to the contraction of other muscles; the vital spirits of the nerves were supposed to flow alternately into its fibres, either on account of valves, which by turns admitted and denied them

a passage; or because it was thought that the spirits could only be discharged by drops, and not in an equable stream, from the extremities of such subtile tubes as the nerves were conceived to be *.

VARIOUS other hypotheses were framed to explain the alternate motion of the heart; a problem not less difficult than curious! These I shall pass over in silence, leaving them to fall by their own insufficiency, or the arguments of others; and content myself with mentioning the defects of some of the later systems, which, from their plausibility, or the high character of their authors, are intitled to the greatest regard.

THE theory of the motion of the heart which has of late years met with the most favourable reception, is that of the celebrated Boerhaave, who deduces the alternate *systole* and *diastole* of that muscle chiefly from the peculiar circumstances of the cardiac nerves; for as the greatest part of those nerves passes between the auricles and large arteries of the heart, he concluded that they must be compressed at the end of every *systole*, when these cavities and vessels are greatly distended with blood; whence the motion of the spirits being intercepted, the heart must be rendered paralytic; but that whenever, upon the subsequent contraction of the auricles and arteries, this compression ceases, and the nerves transmit their fluid as formerly, the heart must contract anew †.

THIS hypothesis, however ingenious, will appear insufficient, if we attend to the following considerations.

I. ALL the cardiac nerves do not pass between the auricles and arteries. Not to mention many smaller ones, there are two considerable branches distributed to the muscular substance of the heart, which neither pass between the two auricles nor the two arteries; and therefore cannot be liable to any alternate compression from them.

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* Borell. de mot. animal. lib. 11. cap. 6. prop. 79.

† Institut. med. No. 409.

2. I believe it will be difficult to persuade anatomists, that the nerves (supposing that they all had their course between the auricles and arteries) could suffer any such compression as is here supposed; considering the softness of the parts, and the fat upon the external coat of the arteries and auricles, which must defend them in a great measure from it: besides, should we not observe other muscles of the body become alternately or constantly paralytic, whose nerves run contiguous to any considerable artery, or are compressed by any preternatural tumor? “Qui fit, obsecro,” says the illustrious Morgagni, “ut nervi intercostalis munera ab assidua arteriæ carotidis pulsatione non turbentur, præsertim cum is nervus non possit cedere, sed communis ipsi et arteriæ ossei foraminis parietibus allidatur? Qui fieri posset, ut in Veneta muliere, quam cum amicis dissecuimus, cum arteriæ subclaviæ sinistræ superiores posticque parietes in aneurisma expansi, duos tresve nervos ex iis qui ab inferioribus cervicis vertebrae ad brachium descendunt nulla prorsus interposita re contingerent; qui fieri, inquam, posset, ut nulla tamen debilitas, nullus torpor in eo brachio fuerit animadversus *?”

WHY are not all the *viscera* in the abdomen, to which the intercostal nerves are distributed, agitated with alternate contractions answering to those of the diaphragm, since these nerves, by passing through the fleshy part of this muscle, must be liable to compression every time inspiration is performed? And why do not many of the voluntary muscles, when strongly contracted, occasion palsies or stupors of the parts below them, by pressing upon the nerves to which they are contiguous?

3. IT is to be remarked, that a slight compression of a nerve is not sufficient to render the muscle to which it belongs paralytic: thus the ulnar nerve must be pretty strongly compressed against a hard bone, before the ring and little fingers are deprived of their power of motion; nor does this happen without being attended with a disagreeable sensation in these parts.

4. As

* Morgagni Adversar. anat. vi. animad. 24.

4. As the compression of the ulnar nerve does not immediately bring a palsy on the muscles of the fingers now mentioned, but after it has been continued for some considerable time; so when this pressure is removed, the motion of these muscles does not return immediately and all at once, but by degrees; and therefore, allowing the cardiac nerves to be alternately compressed by the auricles and arteries, yet the heart ought not to be rendered instantly paralytic by such compression, nor should it recover its motion as soon as this compression is removed. But further,

5. GRANTING that the cardiac nerves suffered as great compression in their passage between the auricles and arteries to the heart, as the favourers of this opinion could desire; what will follow? an effect surely different from that which is here contended for; since the immediate consequence of such compression must be the squeezing forward towards the heart the spirits supposed to be contained in that portion of the nerves which is below where the compression is made; for if the nerves are supposed to be hollow tubes which convey a fluid to the heart in order to its contraction, the first and most immediate effect of their being compressed by the dilatation of the auricles and arteries, must be a quicker propulsion and more copious derivation of the spirits into its fibres; *i. e.* the heart ought to be most strongly contracted at the time its *diastole* is observed to begin. And in fact we find, that a ligature made on the *par vagum* is so far from making the heart immediately paralytic, and preventing its contraction, that it causes strong convulsive motions or palpitations of that organ; although these motions, indeed, are rather to be ascribed to the irritation which those nerves suffer than to any propulsion of the spirits (supposed to be contained in them) towards the heart *.

6. THIS supposed alternate compression of the cardiac nerves cannot account for the motion of the auricles, whose *systole* happens when their nerves ought to be compressed, and consequently when

* See below, Sect. xiv. No. 26. 7

the derivation of the spirits into them should be intercepted. If it be said, that the auricles are ready to contract when the *systole* of the ventricles begins, but that being weaker muscles, they must wait till the *systole* is finished * : I answer, that, if this were true, the auricles should become pale and tense while the ventricles are contracted, since, when the influence of the nerves acts more strongly on any muscle, it becomes equally hard, whether it be allowed to contract, and its extremities to approach each other, or not. But further, as an influx of spirits into the fibres of any muscle must be immediately followed by an endeavour in them to contract, so, if this be prevented, as soon as the spirits are again intercepted, their influence to produce any contraction will cease. This is the case with such muscles as are under the power of the will, where any sudden but not continued effort, if it is not allowed that instant to take effect, immediately vanishes; nor is it to be doubted, that the same thing must happen to the auricles of the heart. But, be this as it will, it is evident, that the alternate motions of the auricles cannot be owing to any compression of their nerves; since it is acknowledged, by the best anatomists, that the course of these nerves is such as cannot subject them to any alternate pressure; which is also true of all the cardiac nerves in those animals whose hearts have only one ventricle.

7. IN dying animals, the right ventricle continues to contract after the left ventricle has ceased, and the right auricle performs its motions for some time after its ventricle †. But these alternate motions of the right ventricle and auricle, cannot arise from any compression of their nerves; since, in the first case, neither the *aorta* nor the left auricle are dilated with blood at the end of the *systole* of the right ventricle; and in the latter, the pulmonary artery also remains empty. In frogs and fishes, whose heart has but one ventricle and one artery going out from it, the alternate motions of this organ are as regular as in men and quadrupedes: and it is well known, that

* Bellin. de motu cord. prop. 2. Keil's anatomy cap. 3. sect. iv.

† Harvey de mot. cord. et sang. cap. 4.

that the hearts of many animals, after being being separated from their bodies, continue for some time to be alternately contracted and relaxed with great regularity, when there can be no alternate compression to intercept the nervous influence at the end of every *systole*.

8. LASTLY, It may well be looked upon as a defect of this theory concerning the motion of the heart, that it does not assist us in explaining the spontaneous action of other organs in the body, whose nerves cannot be supposed liable to an alternate compression.

SOME have imagined, that, as the intercostal nerves pass through the same holes of the *cranium* with the carotid arteries, they must, therefore, suffer such compression from the *diastole* of these arteries as shall render the heart paralytic at the end of every *systole*. In answer to which, it may be sufficient to ask, why the other muscles and *viscera* which receive nerves from the intercostals, do not exactly correspond with the heart in their motions; or why the auricles and ventricles of the heart are not contracted and relaxed at the same time?

THE learned De Gorter, fully aware that the supposed alternate compression of the cardiac nerves afforded no satisfactory account of the motions of the heart, supposes that vital or involuntary motion is owing to one and the same cause, both in the heart and other organs of the body: this cause he imagines to be such a structure of the involuntary muscles, that, when their fibres are dilated by the spirits, the small nerves which pass between them are compressed; so that no sooner are the fibres inflated, than the spirits are intercepted, and consequently the muscle begins to be relaxed; but by this relaxation of the muscular fibres, freeing the nerves from compression, the spirits are transmitted as formerly, and the muscle is contracted anew. And in this manner he fancies that, as long as life remains, the muscles of involuntary motion must be alternately contracted and relaxed*. But,

1. BESIDES, that this structure of the vital organs is merely hypothetical, and unsupported by any experiment, or microscopical observation,

* Gorter de motu vitali, sect. 39.

observation, it may be asked, why all the vital organs are not contracted and relaxed at the same instant; or at least why the motions of some are renewed after shorter, and of others after longer intervals?

2. IF such were the structure of the muscles of spontaneous motion, that their contraction must be immediately followed by their relaxation, how comes it, that, by an effort of the will, we can keep the diaphragm in its strongest state of contraction as long as we please? and why does not the relaxation of this muscle necessarily follow its contraction, if its alternate motions depend on a general structure, common to it with the heart and intestines?

3. IN cases where the lungs are obstructed and respiration is rendered difficult, we find, that, even in time of sleep, other muscles besides the common inspiratory ones are brought into alternate contractions, in order to raise the ribs, and enlarge the cavity of the *thorax*; whence it appears, that muscles of the voluntary kind may, on certain occasions, be employed in the performance of the vital motions, altho' there be nothing in the structure of these muscles peculiarly fitting them for such alternate motions.

4. FURTHER, the pupil (whose motions are as involuntary, and as little perceived by us as those of the heart) is not immediately relaxed, after having been contracted by the admission of light into the eye, but it remains in the same degree of contraction as long as the same quantity of light is transmitted to the *retina*; which could not happen, if any such structure really obtained in the muscles of the *uvea*, as De Gorter supposes in the muscles of involuntary motion. We must therefore set aside his theory, not only as a mere hypothesis, but as insufficient to explain the various appearances of spontaneous motion.

SOME ingenious physiologists have imagined the contraction of the heart to be owing to the elastic power of its fibres, which, after they have been stretched by the returning venous blood dilating the
auricles

auricles and ventricles, refile, like a bent bow, with a considerable force. But the force with which a spring recoils is ever proportional to the power which bent it; and therefore, since the sides of the heart contract with a much greater power than that with which they were forced asunder, the *systole* of this muscle cannot arise merely from the elasticity of its fibres, but must be owing to some additional *impetus*, at that time, communicated to them.

THIS much being premised, in order to shew the insufficiency of some of the most plausible theories that have hitherto appeared concerning the heart's motion; we shall endeavour, in the following section, to give such an account of its *systole*, as it is hoped will appear no less supported by reason and analogy, than founded on experiment and observation.

S E C T. III.

Of the systole of the heart.

BEFORE we inquire into the causes of the alternate motions of the heart, it will be proper briefly to mention and describe three different states of that muscle, *viz.* its contraction, relaxation, and dilatation; of which the first and last may be said to be violent, and the second only natural to the heart. During its *systole*, the heart is contracted in all its dimensions *, and its substance becomes remarkably hard: This state, which scarcely lasts one third of the time interveining between each contraction, is followed by a general relaxation of the heart, by which this muscle becomes soft and flabby, and somewhat longer. Bartholine calls this the *perisystole* of

* It has been disputed, whether the heart becomes shorter or longer in the time of its *systole*. But, after carefully inspecting the hearts of frogs and several other animals, both in the body, and when separated from it, I cannot help (notwithstanding the authority of Winslow on the other side) agreeing with Dr Hunauld and others, who affirm, that the heart is diminished in length, as well as in breadth, when it begins to contract. Vid. *Histoire de l'acad. des sciences* 1731, edit. 8vo, p. 33. &c.

of the heart *. It continues a shorter time than the *systole*, the ventricles being, instantly after their relaxation, filled with the returning venous blood, and distended much beyond their natural capacity, or that which they are observed to have in animals newly dead, when the fibres of the heart are neither contracted nor dilated by any adventitious force, but left intirely to themselves. The *diastole* of the heart being thus completed, its *systole* immediately ensues.

SUPPOSING the heart now in its full *diastole*; let us inquire what change has happened to it since the end of the preceeding *systole*, which may be supposed capable of bringing it into a new contraction. We have already shewn, that the nerves of the heart are not, at this time, freed from any compression which a little before could have rendered it paralytic. And if one should suppose some general structure in the brain, which determines the vital spirits through its nerves alternately, and as it were in succeeding waves, yet this would not account for the motions of the heart; since its alternate contractions continue for some time after all communication between it and the brain has been cut off. Further, as the contractions of some of the organs of vital motion are performed after shorter, of others after longer intervals, we must suppose, at the origin of the nerves belonging to each organ, a different cause alternately determining the spirits into it.

DURING the *diastole* of the heart, all its coronary vessels, which were in a great measure emptied by the preceding *systole*, are filled with blood violently pushed into them by the contraction of the *aorta*: but as the arterial blood is not immediately necessary to the contraction of a muscle †, and seems only to contribute to it in a secondary way, this alone will not be thought sufficient to account for the succeeding *systole* of the heart. 'Tis true indeed that warm water, injected into the arteries of an animal newly dead, excites some kind of motion in the muscles to which these arteries

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* Anatom. p. 37. 377. 378.

† Sect. 1. No. 2. of this Essay.

are distributed. But this contraction is so weak and so unlike that of the heart, that from this experiment alone, none can well imagine the *systole* of the heart to be owing to the arterial blood pushed forcibly through all its vessels, in the time of its *diastole*; especially as we do not observe any alternate motion in the sphincters of the *anus* and bladder, from the blood being more strongly impelled through their vessels, upon every contraction of the heart, than during its *diastole*. This matter however is put beyond dispute, by the heart's continuing to repeat its contractions, not only after the coronary arteries and pulmonary veins are tied, but after it is separated from the body. The blood then with which the coronary vessels of the heart are filled during its *diastole*, being as insufficient as the supposed compression of the cardiac nerves to account for its succeeding *systole*; it remains to inquire, what influence the returning venous blood, with which the ventricles of the heart are distended during its *diastole*, may have in producing its subsequent *systole*. And is it not reasonable to suppose, that this fluid returning by the *cavæ* and pulmonary veins, and rushing into the cavities of the heart, with a considerable force, must, by distending its fibres, as well as by its motion and attrition upon the scabrous surface and fleshy pillars of the ventricles, so stimulate and affect the nerves of the heart as to bring it immediately into contraction *?

THOUGH some authors have long since ascribed the alternate motion of the heart to the irritation of the blood received by turns into its cavities †; yet as this cause has been little attended to by several of the later writers, and not rightly understood by others, we shall endeavour to ascertain its influence by a variety of arguments.

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WHILE

* Sect. 1. No 8. 9. &c.

† Certumque est, vesiculam dictam, ut et cordis auriculam postea, (unde pulsatio primum incipit), a distendente sanguine, ad constrictionis motum irritari. Harvey de generat. animal. exercit. 51.

Fibræ cordis, virtute micationis vitalis sanguinis in ejus ventriculis contenti, per vices IRRITATÆ, excitantur ad se contrahendas et pulsationem faciunt, MOX IRRITATIONE remissa relaxantur. Glisson de ventricul. et intestin. cap. 7. p. 170.

WHILE some authors have ascribed the contraction of the heart solely to the blood, considered as a stimulating fluid irritating the internal surface of its ventricles; others have been unwilling to allow, that the blood acts in any other sense as a *stimulus* upon the heart, than by its weight and impulsive force stretching and extending the fibres which compose its ventricles *. But the increased motion of the blood, from the contagion of the small pox, measles, &c. from eating or drinking any thing acrid, and the power which acrid or stimulating substances have in reviving the motion of the heart after it is separated from the body, are circumstances which shew, that the contraction of the heart is not solely owing to its fibres being distended by the *momentum* of the blood, but partly to the irritation communicated to its internal surface by the particles of that fluid. And the sensible diminution of the peristaltic motion of the bowels, when the cystic bile is hindered from flowing into them, makes it evident that the stretching of the fibres of the intestines, by the air and aliment contained in them, is not the sole cause of their succeeding contraction. On the other hand, the increase of the motion of the heart from exercise, or from any other cause, by which the blood is returned in greater quantity, and with more force; its diminution by bleeding, the motion of the stomach, and the expulsion of the urine and *fæces* †; all these particulars, I say, prove, that even the distension of hollow muscles has a remarkable influence towards exciting them into action ‡.

FARTHER, that the blood is well fitted to act upon the heart as a *stimulus*, in both these ways, will appear, if we consider its composition, its heat and intestine motion, that which some authors have supposed it to receive from the air, and the force with which it rushes into the cavities of the heart.

1. As to its composition. The blood consists of the same principles with our aliments, and consequently abounds with salts and oils. The salts of the blood are partly of the fixed neutral kind, and

* Senac. Traité du COEUR, lib. 2. cap. 4. sect. 4.

† See sect. 5. below.

‡ See sect. 1. N 8. above.

and partly such as are rendered semivolatile by the heat and motion to which they are subjected; both are apt to irritate the sensible nervous parts; for we know that any kind of salt applied to the eye gives great uneasiness. The oils in the blood are either those of animal substances, the expressed oil of vegetables, their attenuated oil by fermentation, commonly called *alcohol*, or lastly the acrid oil of aromatics. The two first are not acrid, nor fit to act as a *stimulus*, unless they have been much attenuated by long exposition to heat, or by attrition; the two latter, *viz.* ardent spirits and the oil of aromatics, are very apt to irritate the tender fibres of living animals. Hence it is that spirituous liquors largely drunk, or hot spices too freely used, soon raise the pulse, and make the heart as it were redouble its contractions. Hence animal food or strong liquors, which abound with saline and acrid particles, quickens the circulation, and increases the heat of the body, while milk, mild herbs, or cooling fruits, make little alteration in the pulse. The blood therefore, as containing salts and attenuated acrid oils, must be well fitted for communicating a gentle *stimulus* to those sensible nerves which terminate on the internal surface of the auricles and ventricles of the heart.

If it be objected, that the blood discovers no acrimony to the tongue, nor sensibly irritates the eye; it may be sufficient to answer, that, though this fluid be sensibly salt to the taste, yet I do not ascribe the whole stimulating power to its acrid particles alone, but to those in conjunction with other qualities and circumstances which I shall presently mention. But further, although the blood did not discover the least degree of acrimony when applied to the nerves of the tongue, yet it might be well fitted to act as a *stimulus* upon other nerves of the body, differing from these in their constitution and peculiar sensibility. Many poisons, especially those of the antimonial kind, are almost free from any degree of acrimony, as far as we can judge by the taste; yet they so strongly and disagreeably affect the nerves of the stomach, as to bring it with the neighbouring parts into violent convulsions. The roots of the *cicuta aquatica* are sweetish, but neither acrid nor disagreeable; and cataplasms of

them applied to inflamed or ulcerated parts, occasion no bad symptoms * ; yet, when taken into the stomach, they soon throw the whole body into such convulsions as generally end in death. The berries of the *rhus myrtifolia Monspeliaca*, though they have nothing in their taste or smell to make them be suspected, yet act so powerfully upon the nerves of the stomach, that in half an hour they bring on epileptic fits, which kill the person who has swallowed them in less than 24 hours †. Viper's poison affects neither the nerves of the tongue nor stomach with any disagreeable sensation; yet the smallest drop of it received by a wound into the blood, seems not only to act as a ferment upon it, but, by its stimulating quality, to affect the whole nervous and vascular systems. The putrid excrement which gives no disturbance to the *colon* or *rectum*, till by its quantity it overstretchs their fibres, would occasion sickness and vomiting in the stomach. Urine, which scarce stimulates the bladder till it begins to distend it too much, proves a laxative, when given by way of clyster. Blood received into the stomach by a rupture of its vessels, is offensive to its nerves, but does not affect the nerves of the heart or arteries with any disagreeable sensation. The changes which happen in the body about the time of puberty are generally, and not without reason, ascribed to the *semen* which then begins to be duly prepared: they do not, however, seem to be so much owing to the reception of the finer parts of that fluid into the blood, as to its peculiar action upon the nerves of the *testes* and *vesiculæ seminales*; yet the *semen*, when applied to the nerves of other parts of the body, neither sensibly excites titillation, nor produces any remarkable effect.

THUS it appears, from a variety of examples, that the nerves of different organs in the same animals are so constituted as to be very differently affected even by the same things: So that we cannot absolutely judge, by our taste or smell, how far any liquor may or may not be adapted to act as a *stimulus* upon the nerves of a particular organ. We may then conclude, that altho' the blood scarce
sensibly

* Wepferi Historia cicut. aquat. p. 84.

† Memoires de l' Acad. des sciences 1739. edit. 8vo. p. 627.

sensibly irritates the eyes or tongue, it may nevertheless so stimulate the nervous *papillæ* of the heart as to excite that muscle into contraction: which will further appear, if we consider the heat of this fluid.

2. HEAT seems to be no more than a quick vibration or motion in the smaller parts of bodies; therefore the blood, as it is a warm fluid, will have its particles agitated by perpetual vibrations, which must be communicated to the nervous *papillæ* on the internal surface of the heart: besides, as the blood abounds with oily and sulphureous particles, it will, by its motion and attrition against the vessels, acquire vibrations still more remarkable.

BECAUSE in fishes and frogs, whose hearts, when separated from the body, long retain their moving power, the blood is but little warmer than the water in which they swim, it has been said, that the stimulating quality of the blood cannot be owing to its heat; but upon a little reflection we shall see the fallacy of this conclusion.

IT will not be denied that a certain degree of heat is necessary to the continuance of the motion of the heart in those animals whose blood is warm; and the same thing is not less true of frogs and fishes, whose blood is not really cold, although it may be said to be so, when compared with that fluid in men and many other animals. As a proof of this, not only bats, hedgehogs, and most of the insect tribe remain, in northern countries, in a torpid state during the winter, but the same thing happens to frogs and other animals, whose blood is but very little warmer than the air or water which surrounds them. The motion of the heart, however, even in these animals, may be renewed at any time, and the circulation of the blood and life may be restored, by exposing them to an equal or greater heat than that of summer; from which it follows, that a certain degree of heat is necessary, even in the coldest animals, for the continuance of the motion of the heart: and as a further proof, when the hearts of frogs and fishes, after being separated from their bodies, begin to move more languidly, we shall find them acquiring
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new vigour by being exposed to the heat of a fire, or even to the sun-beams in a hot day.

THE truth is, there is no absolute cold, but only different degrees of heat; and blood whose heat does not exceed the fortieth or fiftieth degree in Farenheit's scale *, may be as fit to stimulate the heart in certain animals, as the much hotter blood of men and quadrupedes may be to continue the motion of that organ in them. And indeed, the remarkable effect which heat has in exciting the hearts of animals into alternate contractions, and thereby promoting the circulation of their fluids, is too well known to admit of any doubt. By different degrees of warmth or cold, insects, and even the chick in the shell, may at pleasure be made more or less lively, may be left to die or restored to life †.

3. THE particles of the blood, besides the oscillations they are put into by heat, are agitated by a motion of another kind. As vegetables make up the chief part of our aliment, the chyle is generally acedcent; yet the blood and other elaborated animal fluids are of a contrary nature, ever tending to putrefaction, and by the fire, afford a volatile instead of a fixed alkaline salt: such a change in the nature of the chyle could not, I think, be produced without an intestine motion, which we find to be the grand instrument of nature in changing the texture and qualities of all vegetable and animal bodies ‡. This intestine motion added to the vibrations of the particles

* It is to be observed, that although, in northern climates, the surface of the sea is often colder than fresh water when it begins to freeze, and consequently would make Farenheit's thermometer fall under thirty-two degrees, yet at any considerable depth, and where fishes generally live, the sea is not much colder in winter than in summer.

† Vid. Harvey de motu cordis, cap. 17. de generatione animal. exercitat. 17.; et Reaumur. Histoire des insectes, tom. 2. memoire 1.

‡ It does not seem to be more reasonable to deny any degree of intestine motion in the fluids of animals, because this is not perceived by our senses, than it would be to argue against their being possessed of any degree of heat, because we are not sensible of their warmth when moving through our vessels. A fluid, such as the blood, composed of various and disagreeing particles, whose attractive and repulsive powers are different, and upon which the same degrees of heat and friction must have different effects, will probably, by its rapid motion, have its globules and their constituent parts agitated with brisk vibrations.

ticles of the blood from heat, will still better qualify it for acting as a *stimulus* upon the extremely sensible and tender nerves of the heart.

4. SIR Isaac Newton imagined, that the beating of the heart was continued by an acid vapour in the air, received into the blood by means of respiration *; in which opinion he has been followed by Dr Robinson † and others. But although particular regard is to be paid even to the hints and conjectures of so great a man, especially of one who was no less remarkable for his caution in advancing hypotheses, than for his deep knowledge of nature; yet the love of truth is ever to be preferred to the greatest names.

THE opinion of Sir Isaac Newton, as well as other philosophers, who imagined that fresh air was necessary to preserve the life of animals, on account of some *pabulum* or vivifying spirit which it contains, seems to have taken its rise from observing the death of such animals as were confined in a close vessel, and consequently were obliged, for a long time, to breathe the same air. But this fact is not a sufficient proof of that opinion: and were it not foreign to my present purpose, I think that I could bring good arguments to prove that the death of animals in air unrenewed, is not owing to the consumption of any vivifying spirit or *pabulum vite* which it has been supposed to contain, but to the vapours exhaling from their bodies, and particularly from their lungs, which soon acquire such a putrid acrimony as to become as much a real poison to the nerves of the bronchial tubes and pulmonary vesicles, as is the vapour of the *Grotta del Cane* in Italy, the damp of subterraneous places, or the steams of burning charcoal.

5. A body, whether fluid or solid, of a nature which qualifies it to act as a *stimulus*, will excite so much the stronger irritations, by how much greater the force is with which it is applied to the sensible part; since its acute, acrid, or otherwise active particles, must by this means strike more strongly against the extremities of the nerves.

* Optics, edit. 8vo, p. 355.

† Treatise on the animal œconomy, prop. 24.

nerves. Hence the blood, which we have shewn to be qualified for gently irritating the sensible membranes of the cavities of the heart, must, by its being thrown into them with a considerable force, act with so much the greater energy. But further, as by the blood rushing impetuously into the auricles and ventricles of the heart, these cavities are dilated beyond their natural capacity, so the extension which their fibres suffer on this occasion will produce some sort of irritation, and thus prove a *stimulus* to their subsequent contraction *. Agreeably to this, Wepferus has observed, that after one vermicular contraction of the stomach, another does not succeed, till that organ begins to swell in its middle part, by the rarified air contained in it or generated by the dissolving aliments : But this distension of the stomach no sooner happens, than a new contraction of it begins, which proceeding on towards the *pylorus*, pushes part of that air and of the digested aliment into the *duodenum* ; after which this orifice collapses, and a new intumescence of the stomach ensues †. Hence it appears how great an analogy there is between the causes of the alternate contraction of the heart and stomach ; both being excited, partly by the irritation of their fibres by a distending cause, and partly by the irritation of their nerves by a stimulating one. In like manner the contraction of the bladder of urine, and the desire of evacuating this fluid, is not only owing to its acrimony stimulating the nerves of the bladder, but also to its quantity overstretching its coats, and irritating its fibres.

UPON the whole, from what has been said, it may appear, that as the violent motion of the fluids, and uncommon contractions of the heart and arteries in the small pox, measles, and other diseases,

• It here deserves notice, that while the ventricles of the heart are extended much beyond their natural size, by the force of the reflux venous blood, the tendineo-carnous chords running from one side of the ventricles to the other (*a*), will be considerably stretched : this ought to produce an irritation in these parts, and consequently contribute towards exciting the succeeding *systole* of the heart.

† Wepferus de cicut. aquat. p. 177.

(*a*) Vid. Cowper's Myotom. reformat. tab. 39. let. *b*. et tab. 40. let. *f*. also Senac. Traité du coeur, vol. 1.

eases attended with fever, is in a great measure owing to some foreign particles mixed with the blood, by which their nerves are more strongly stimulated; so the ordinary and less violent motion of the heart is owing to the gentler *stimulus* of the fluids in a sound state.

THAT the alternate contractions of the heart are excited in the manner above explained, a variety of other arguments concur to shew.

1. THE quickness and strength of the motion of the heart are, *ceteris paribus*, proportional to the force with which the venous blood returns to its ventricles by the *venæ cavæ* and pulmonary veins: hence exercise of any kind accelerates the motion of the heart, and increases the force with which it contracts: a fit of laughter will quicken the pulse above twenty strokes in a minute*: upon an intermission of respiration, the pulse becomes smaller, but recovers its former strength immediately after breathing again†.

2. IT appears from Dr Hales's experiments, that the blood returns to the heart by the two *venæ cavæ* with nearly $\frac{1}{5}$ of the force with which it was pushed into the *aorta*; and as the left ventricle of the heart is at least three times stronger than the right, the returning venous blood will endeavour to dilate the right ventricle with a force nearly equal to $\frac{1}{3}$ of the power with which it usually contracts in the time of its *systole*; and this even without taking into the computation the additional *impetus* communicated to the blood by the contraction of the right auricle: but, by violent straining, the force of the blood in the veins is often made above four times greater than ordinary‡, and consequently superior to that with which the right ventricle contracts when the body is at rest: therefore, if we do not allow the strength with which the ventricles of the heart contract to depend in a great measure upon

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* Robinson on the animal œconomy, prop. 21.

† Ibid. prop. 24.

‡ Hales's Statical essays, vol. 2. p. 14. and 161.

the action of the venous blood on them, it will be hard to conceive how the right ventricle should be able to overcome the force with which the blood rushes into it, upon any straining or violent exercise, and in horses running at full speed. Moreover, it is evident, from the state of the pulse in peripneumonies, both before and after bleeding, as also from the increase of the force of the blood in the *aorta* and its branches after deep sighing*, that the strength with which the left ventricle of the heart contracts is immediately increased or diminished, according as the blood is squeezed with more or less force through the pulmonary veins into its cavity.

3. IT is observable, that the auricles and ventricles of the heart are no sooner filled with the reflux blood, than they immediately begin to contract; which shews that the influx of this fluid is the cause exciting their subsequent contraction. In dying animals, those cavities of the heart cease from motion first, which are first deprived of the returning venous blood: hence a little before death, when the blood is not pushed by the force of the right ventricle beyond the capillary arteries of the lungs, the left ventricle being deprived of its *stimulus*, is observed first of all to cease from motion, and soon after it, the left auricle: but the right ventricle, being still supplied with blood from the two *cavæ*, continues its motion for some considerable time; and, even after it seems to die, the alternate motions of the right auricle are kept up by the small stream of blood which flows into it from the *venæ cavæ*. This, however, is not sufficient to actuate the right ventricle, till after several contractions of its auricle, more blood being collected in it, it begins anew to tremble, and, as it were with some struggle and difficulty, slowly performs another contraction †. After both the

* Hale's Statical essays, vol. 2. p. 6. and 16.

† After cutting off the *venæ cavæ*, opening the pulmonary artery, and emptying the right ventricle of the heart of all the blood contained in it, M. De Haller tied the *aorta*, by which means the left ventricle and auricle were kept full of blood. He then observed the right auricle to remain at rest, while the right ventricle was affected with a feeble palpitation in consequence of its sympathy with the left one: But the left auricle beat strongly for some time, and the left ventricle continued its alternate motions for near two hours. See Act. Gottingenf. vol. 1.

the right ventricle and auricle have wholly lost their motion, the right *sinus venosus* continues for some little time gently to palpitate, and its tremulous motion, when about to cease, may, like that of the heart, be renewed by heat, or any thing else that is capable to irritate its fibres *. After this right *sinus venosus* has also ceased to move, the trunks of the *venæ cavæ* adjoining to it continue, for some time, to be alternately contracted and relaxed. When the *vena cava inferior* is tied, and the blood is squeezed out of that portion of it which is between the ligature and the heart, its motion immediately ceases, but begins again as soon as the ligature is removed and new blood is allowed to flow through that vein. Lastly, after both the *venæ cavæ* of a rabbit were tied, and the blood contained in them and the heart was evacuated by a hole made in the substance of this organ, its motions, as well as those of its auricles, and the trunks of the *venæ cavæ*, ceased at once, but were renewed upon the ligatures being taken away †.

DR LANGRISH informs us, that in a dog whose *thorax* he opened, and whose lungs he kept in action with a pair of bellows, the auricles began the motion, and the *systole* of the ventricles always followed that of the auricles. When he desisted from blowing fresh air into the lungs, the heart lay still, but recovered its motion when the lungs were distended anew. In this action, he never could discover that the ventricles began the motion, but the auricles always contracted first, and immediately after them, the ventricles; though at last he observed several contractions of the auricles which were not succeeded by any motion in the ventricles ‡. From what has been said, it appears, why the motions of the auricles and ventricles are not synchronous, *viz.* because they receive into their cavities at different times the returning blood, which, as a *stimulus*, excites them into contraction.

* Harvey de motu cord. cap. 4.; et Walæus de motu chyli et sang. epist. 1. ad fin. Bartholin. anat. p. 783. et 784.

† See Bartholin. epistol. Anatom. cent. iv. p. 111. et 112.

‡ Cronean lectures on muscular motion, p. 61. 62.

4. PEOPLE frequently recover from a *leipothymia* and *syncope* as it were spontaneously, and without any external assistance, because the chyle and lymph continue, by means of the peristaltic motion of the guts, to be forwarded to the subclavian vein and *cava*; at the same time that the venous blood, partly by the contractile power of the greater arteries, and the oscillatory motions of the smaller ones, and partly by the constriction of the cutaneous vessels from cold, is transmitted into the branches of the two *venæ cavæ*, and forwarded to the right auricle of the heart, which it first stimulates into contraction; and immediately afterwards sets the right ventricle also a going. Nay, some who have been dead in appearance, have been restored to life by having air blown into their lungs, which communicated a new motion to the stagnating blood in the *cava inferior* and pulmonary veins. Of this we have a memorable instance in the Edinburgh Medical Essays, vol. 5. art. 55.; where we are informed, that a man was brought to life, by distending his lungs with air, and putting the blood in the pulmonary veins and left *sinus venosus* into motion, after his heart had remained at rest for at least half an hour*: and that it was in this way that the blowing into his lungs recovered him, is evident; since no sooner were the lungs thus dilated, than immediately the heart began to move, and six or seven very quick pulsations were felt in his left breast; after this, the lungs of themselves continuing to act, a pulse was soon perceived in the arteries. Hence it appears, that, in order to put the heart in motion and restore life in animals which are not irrecoverably dead, it is only necessary to communicate such a motion to the blood in the *cava* or pulmonary veins, as may enable it a little to dilate the auricles and ventricles of the heart.

5. THE heart after it has ceased to move, is not only put in motion again by determining the venous blood into its cavities, but, in animals which have been for some time dead, its action may be renewed by blowing air through the thoracic duct or *vena cava* into

* The inflation of the lungs, by pressing the *vena cava inferior*, must also have communicated a motion to the blood in the right *sinus venosus*.

to its right auricle and ventricle, or through the *aorta* into its left ventricle. Thus, while Peyerus was endeavouring to distend the *receptaculum chyli* and thoracic duct with air, the heart was not only rendered turgid by the lymph which had made its way into it, but immediately began to move, and continued in motion for several hours *. The same experiment was afterwards repeated by Brunnerus on a dog with equal success †. And Harderus relates, that in a stork, which had been killed by poison, he made the heart renew its motion, by blowing air into the *aorta* ‡. Since, in these experiments, and in others which might be recited from other authors, the heart, which had lain quiet, and without any motion for a considerable time after death, was readily excited into contraction by the air stretching its fibres, and probably stimulating its nervous *papillæ*; and since the heart, as we are told by Dr Harvey, and as I have often experienced, after it has ceased to move in an animal newly killed, may be again put in motion, by applying a little warm *saliva* ||; we need not be at a loss to account for the alternate motions of that organ in living animals, where a warm active fluid is alternately thrown into its cavities.

ONE objection, only, at present occurs to the above account of the contraction of the heart, *viz.* that its alternate motions may be owing to some peculiar power resulting from the structure and constitution of its fibres, and that by virtue of this, it is enabled to continue these motions long after the blood has ceased to act upon it. In answer to which, it is sufficient to observe, that in dead animals in whom the motion of the blood is stopped, the heart remains at rest till its vibrations are renewed by exposing it to the open air, or by otherwise stimulating it **: whatever power therefore may be supposed

* Peyerii Parerg. 7. p. 199. and Wepfer. de cicut. aquat. p. 89.

† Experiment. circa pancreas, p. 21.

‡ Additamenta ad Peyerii Parerg. 7. p. 201.

|| In columba certe experimento facto, postquam cor desierat omnino moveri, et nunc etiam auriculæ motum reliquerant per aliquid spatium, digitum saliva madefactum, et calidum cordi superimpositum detinui: hoc fomento quasi vires et vitam postliminio recuperasset, cor, et ejus auriculæ, moveri et sese contrahere atque laxare, et quasi ab orco revocari videbantur.

Harv. de motu. cord. cap. 4.

** Sect. xiv. below.

supposed to reside in the fibres of the heart, a *stimulus* of one kind or other is always necessary to excite it into action. In living animals this *stimulus* is, as we have shewn, no other than the returning venous blood: in animals newly dead, warm water, air, and a variety of other *stimuli* excite into action this power which seems to reside in the fibres of the heart; which, whether it is owing merely to their mechanical structure, to the animal spirits lodged in them, or to some other cause, will more fully appear in the sequel of this Essay.

S E C T. IV.

Of the relaxation and diastole of the heart.

HAVING shewn that the *systole* of the heart is owing to the returning venous blood acting upon its auricles and ventricles as a *stimulus*, it remains that we next inquire in what manner its relaxation and *diastole* are brought about.

THE ventricles of the heart having by their contraction, expelled the blood contained in them, into the *aorta* and pulmonary arteries, are immediately after relaxed; their fibres losing that tension and firmness which they had the moment before. This relaxation of the heart must necessarily follow its *systole*, since the muscles of living animals, after being excited into contraction by any *stimulus* applied to them, are quickly relaxed again*. What may be the reason of this action we shall not now inquire, but refer it to be afterwards discussed in a more proper place†; it being sufficient for our present purpose that the thing is allowed to be true. Only we may observe with respect to the heart, that as the stimulating cause (*viz.* the blood) is, during the *systole*, expelled out of its cavities, it is reasonable to think that the fibres of this muscle which

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* Sect. i. No. 10. and 11. above.

† Sect. x. below.

were in a violent state, will of themselves endeavour to return to their natural one *.

THE ventricles of the heart, in consequence of the relaxation which happens to their fibres after their *systole* is finished, give no resistance to any cause that begins to dilate them, but will not, without violence, allow their sides to be removed from each other, so much as happens during their *diastole*.

FROM the experiments of the Reverend Dr Hales, compared with those of Dr Langrish, it appears that the capacity of the left ventricle of the heart of an ox, in consequence of the natural relaxation of its fibres, is to its capacity, when fully dilated by the reflux blood, nearly as 1 to $2\frac{1}{2}$ †. As therefore the relaxation of the heart at the end of every *systole*, is owing to the contraction of its fibres ceasing at that time; so its full *diastole* is produced by the returning venous blood, which enters its cavities with a considerable force. Without this, it is impossible that any relaxation of the heart could produce its *diastole*; since a hollow muscle, such as the heart or the bladder, can never be fully dilated by means of its internal mechanism, or without the assistance of a distending cause introduced into its cavities. But although the *diastole*, or full dilatation of the ventricles of the heart, must necessarily be ascribed to the force of the reflux blood; yet this alone, without a preceeding relaxation of their fibres, would be insufficient to produce that effect. 'Tis true indeed, that the contraction of the auricles, and the *momentum* of the reflux blood, are, in some sense, antagonists to the ventricles ‡; but both these taken together falling much short

* Dr Langrish is of opinion, that when the heart is in *systole*, some of its fibres are always stretched beyond their natural tone; so that by their elastic restitutive property, they act in a certain degree as antagonists to the contracted fibres, and so contribute to unbend them at the end of every *systole*; *Cronean lectures*, p. 55. But whatever may be in this, it does not appear that the heart is, by its make, better fitted to relax itself, than the other muscles; since these, or even a few of their fibres, when separated from the body, and so deprived of their antagonists, are observed to be alternately contracted and relaxed like the heart. Vid. Sect. xiv. below.

† Vid Hales's Statical essays, vol. 2. p. 25.; and Langrish's Cronean lectures, No. 147.

‡ It is a mistake to think, that no blood is pushed into the ventricles of the heart during their *diastole*, except what was contained in the auricles properly so called. A certain quantity

short of the force with which the ventricles contract, there must necessarily be some other cause which relaxes the fibres of the heart, and renders them as it were paralytic at the end of every *systole*. Besides this, the flaccid appearance of the heart, immediately after its contraction is finished, and before its ventricles are filled with blood, shews that its fibres are then in a state of relaxation.

WHAT has been said of the relaxation and *diastole* of the ventricles of the heart, is so applicable to its auricles, that it would be superfluous to say any thing of their dilatation.

WE have already observed *, that the force with which the ventricles of the heart contract, is, *ceteris paribus*, proportional to the *momentum* with which the blood flows into them, or, in other words, to the cause dilating them: the *systole* of the ventricles will therefore be, *ceteris paribus*, proportional to their preceeding *diastole*; and hence it is that a full pulse strikes the finger with so much greater force than a small one.

As the left ventricle of the heart must, on account of its superior strength, require a greater force to complete its *diastole* than the right ventricle, the blood ought to return to it with a greater *momentum*; and that it really does so, will, I presume, evidently appear from what follows. The force with which the blood returns to the right ventricle of the heart by the two *venæ cavæ*, is in animals at rest and not agitated with convulsions, according to Dr Hales's experiments, nearly equal to $\frac{1}{10}$ of the force with which it was driven by the left ventricle into the *aorta* †; *i. e.* in a man of an ordinary size, it acts in dilating the right ventricle with a force equal to the pressure of a column of blood whose height is between eight and nine inches, and whose base is equal to the internal surface of that ventricle, *i. e.* with a force equal to the pressure of about five pounds ‡. The force with

tity of blood from the *sinus venosus* also enters them, without being previously received into the auricles: of this the smallness of the left auricle alone is a demonstration. We are therefore to conceive of the blood during the *diastole* of the heart, as rushing into its ventricles both from the auricles and *sinus venosus*, and with the united force arising from its *momentum* in the veins and from the contractile power of these hollow muscles.

* Sect. iii.

† M. Sauvages has computed the *momentum* of the blood in the iliac artery of a dog, to be at least twelve times greater than in its corresponding vein. See *Histoire de l'Acad. de Berlin*, 1755.

‡ Hales's Statical essays, vol. 2. p. 40.

with which the blood returns by the pulmonary veins to the left ventricle of the heart, is not so easily determined; but that it must be considerable, is evident, from the pressure of the air upon the vesicles of the lungs in respiration; the precise force of which as it is difficult to investigate, so it is not to be wondered, if some learned men have fallen into mistakes, when they attempted to estimate it. It seems however probable, from an experiment of John Bernouilli, that when one endeavours to expire with all his force, the whole surface of the vesicles of the lungs may sustain a pressure equal to 420 lib. weight *. But as this can only happen upon the most violent straining, it is of little use to determine the pressure of the air upon the lungs in ordinary respiration; which must bear a small proportion to this, and is not only different in different persons, according to the ease with which they breathe, but very different in the same persons at different times: and although the pressure of the air upon any particular portion of the lungs must appear to be small in ordinary respiration, if we consider how softly, and with what ease this action is carried on, yet the pressure upon the whole internal surface of all the pulmonary vesicles may be considerable. Thus, if the force of the air rushing out at the aperture of the *glottis* in ordinary expiration be supposed equal to the pressure of 2 grains, (which is a small demand), then, since fluids press equally on all sides, every portion of the internal surface of the lungs of the same dimension with the aperture of the *glottis*, *i. e.* every $\frac{1}{8}$ of a square inch of their surface (for the aperture of the *glottis* does not exceed this) must sustain, at that time, a pressure from the air equal to 2 grains; and therefore, supposing the sum of the surface of all the vesicles of the lungs in a man to be 20,000 square inches, it must, in ordinary expiration, sustain a pressure equal to 320,000 grains, or 666 ounces †. If to this force alternately pressing on the lungs, we add the *momentum* which the blood in the pulmonary vessels receives from the contraction of

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* Michellot. de separatione fluidor. p. 181.

† Dr Keil has estimated the sum of the surface of all the vesicles in the human lungs to be 21906 square inches; which computation is in Dr Hales's opinion too low, who has determined this surface in a calf to be 40,000 square inches. Hales's Statical essays, vol. 1. p. 242.

the right ventricle of the heart, it will appear probable, that this fluid returns, by the pulmonary veins, to the left ventricle with a much greater force than it did to the right one by the two *venæ cavæ*.

For a further proof, let us compare the capacities of the two *venæ cavæ* and pulmonary veins. According to the measures of the accurate Santorini, the area of the transverse sections of the two *venæ cavæ*, is to that of the pulmonary veins, nearly as 3 to 2*. Now the *momentum* of the blood in these different vessels must be as the transverse section of the vessels multiplied into the squares of the velocities: but since equal quantities of blood pass through them in equal times, the velocities must be inversely as the sum of the transverse sections; therefore (by compounding *ratios*) the *momenta* will be inversely as the sum of the same transverse sections, *i. e.* the *momentum* of the blood in the *cavæ*, taking Santorini's measures for a standard, is to its *momentum* in the large trunks of the pulmonary veins, as 2 to 3: and this upon the supposition that the resistance to the motion of the blood in the *cavæ* and pulmonary veins were equal; which however is not the case, since the left ventricle of the heart must require a greater force to complete its *diastole* than the right one, and consequently give a greater resistance to the blood flowing into it from the pulmonary veins, than the right ventricle does to the blood in the *cavæ*. Supposing therefore, with Santorini, that the capacities of the *cavæ* and pulmonary veins are generally as 3 to 2, the *momentum* of the blood in the latter will exceed its *momentum* in the former, in a proportion somewhat greater than that of 3 to 2.

M. HELVETIUS, it is true, has drawn a different conclusion from the small capacity of the pulmonary veins, when compared with that of the *venæ cavæ*, or pulmonary artery †, *viz.* that the blood is denser

* Observat. anatom. p. 145.

† The sum of the transverse areas of the pulmonary veins is not only less than that of the two *cavæ*, but also less than the sum of the transverse areas of the branches of the pulmonary artery, contrary to what is observed almost every where else in the body. It has been disputed, whether this discovery was first made by Helvetius or Winslow, or belongs to Drake, who has painted the branches of the pulmonary artery larger and more numerous than those of the veins, (Anat. tab. 12. and 13.); although he says nothing of this inequality either in his

fer in the former than in the latter, but not that its velocity is greater*: and this density, he imagines, it acquires chiefly by being exposed to the cool air in its passage through the small vessels of the lungs. In answer to which, it is sufficient for our purpose to observe, that unless the blood be condensed in the lungs into $\frac{2}{3}$ of its former bulk, (a supposition which cannot be admitted), it must flow through the pulmonary veins with a greater velocity, and consequently with a greater *momentum*, than through the two *cavae*. The small expansion and condensation of water, oil, spirit of wine, and other liquors in thermometers, arising from considerable degrees of heat and cold, shew, that the cool air applied to the surface of the lungs can have but little influence in condensing the blood; besides, it seems not improbable, that the blood may acquire a heat in the lungs sufficient to balance the refrigeration it is there exposed to. It is generally thought, and indeed not without good reason, that the blood in the pulmonary veins is somewhat denser than in the corresponding artery: but this, perhaps, is not so much to be ascribed to the coldness of the air that is inspired, as to its pressure, and to the action of the elastic vessels of the lungs.

If it be objected to what we have offered in proof of the blood's returning with greater force to the left than to the right ventricle of the heart, That in a *fœtus in utero* this seems not to be the case: it may be answered, That the strength of the left ventricle in a *fœtus* exceeds that of the right but little; or, at least, not near so much as in adult animals: that the right ventricle not only pushes part of the blood through the vessels of the lungs, but also distributes a good deal more than $\frac{1}{3}$ of the whole mass to the *aorta* and its branches:

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that

his description of the lungs, or in his explanation of those figures. This debate might have been easily decided, if the persons concerned in it had looked into the *proemium* of Dr Harvey's book *de motu cordis*, &c.; where we find the following passage; from which it appears, that this speciality in the pulmonary veins was not unknown to that illustrious author. "Quum venam arteriosam, vas amplum magnum cum tunica arteriæ factum, non nisi privato
" et uni usui, (viz. alendis pulmonibus), destinarint: cur arteriam venalem *vix pari magnitudine*
" cum tunica venæ molli, laxa, pluribus usibus, tribus vel quatuor videlicet, fabrefactam esse
" asseverant?"

* Memoires de l'Acad. des sciences 1718. edit. 8vo, p. 281. &c.

that the force of the blood returning by the two *cavæ* to the right ventricle is much lessened, by its having a free passage through the *foramen ovale* into the left *sinus venosus*; while the blood by this means enters the left ventricle, not only with the force with which it returns from the lungs, but also with a great part of that with which it flows in the *cavæ*.

IN speaking of the force with which the blood returns to the two ventricles of the heart, we have taken no notice of the additional *impetus* communicated to it by the contractions of the auricles and *sinus venosi*, because this is common to both ventricles; although it must be confessed, that the left auricle seems to be weaker, as well as less capacious than the right; and perhaps it was so formed, as the blood returning with impetuosity from the lungs, after having been intimately mixed and elaborated there, may not require so large an auricle as the venous blood of the *cavæ*, which moves with less force, and is composed of parts less perfectly united.

UPON the whole, if it shall be asked, why the heart, being a solitary muscle, and without an antagonist, does not, like the sphincters, always remain equally contracted? the answer is, that muscles brought into action by a *stimulus*, are immediately relaxed again*; which relaxation therefore happening to the heart, the blood, in its return, enters the ventricles with considerable force, and, by dilating them, acts in some respect as antagonist-muscles do in other parts of the body; and at the same time, by its gentle irritation, it is the cause of their subsequent contraction. The heart must, therefore, be alternately dilated and contracted, as long as the returning blood continues to be poured into its cavities†: nay, since the contractions of muscles from a *stimulus* are alternately repeated, both in living and newly dead animals, although the *stimulus* be not renewed after every contraction‡, and since the heart continues to vibrate for some time after injecting warm water, or air into its cavities, it is probable, that the irritation of the returning blood in a sound

state,

* Sect. i. No. 10.

† Sect. i. No. 12. 13. and 14.

‡ Sect. i. No. 11.

state, is capable of making it perform not only one, but several contractions. This seems to be confirmed by the heart's palpitating after the *vena cava* and pulmonary veins have been tied, and consequently after the blood is intercepted; though indeed it may be allowed, that, in this last instance, the pulsations are either owing to the irritation communicated to the heart by the ligature made on these vessels or to some blood still remaining in its ventricles. Perhaps, when a *stimulus* is slight, it may occasion only one single contraction of a muscle; but when it is greater, it will produce repeated convulsions, and always the more, the stronger it is. The action therefore of the returning blood upon the auricles and ventricles of the heart in living animals, may be supposed not only to excite them once into contraction, but likewise, without the accession of new blood, or any other *stimulus*, to cause some subsequent vibrations, always indeed decreasing in force and frequency; but as in the intervals of these vibrations, the heart is again filled with blood, its alternate contractions, being always stimulated by a new cause, do not become weaker or slower, but continue the same, while the quantity and quality of the blood are unvaried.

It seems therefore no just objection to our account of the motion of the heart, that in many animals newly killed, this muscle, by separating it from the body, or otherwise stimulating it, is excited into alternate contractions, which continue to be repeated for a considerable time, though the *stimulus* be not renewed; since the motions of muscles arising from this cause do not cease immediately upon its removal, but decrease in strength and quickness by slow degrees, before they quite disappear*: nor ought we to be surprised that the violent irritation, occasioned by picking and tearing the fibres of the heart, or from cutting its large vessels, makes it repeat its vibrations more frequently, and continue them for a much longer time, than the gentle *stimulus* of the returning blood.

At what time the motion of the heart begins in nascent animals, and what is the cause which first sets it a-going, are questions not usually

* Vid. Sect. i. No. 11. and Sect. xiv. where the motion of the heart after death, or its separation from the body, is particularly inquired into.

usually inquired into, nor indeed easily answered; although the second seems to admit of a much easier solution than the first.

If all the parts and organs exist in miniature in the animalcule *in semine*, it will scarce be disputed, that while it swims in this liquor, the fluids are propelled through its vessels by the action of its heart, and circulate in the same manner as in the *fœtus in utero*. If the heart does not exist in the animalcule, but is formed after conception, then the beginning of its motion must be later. But, be this as it will, we know that in impregnated eggs, the animalcule lies in a death-like state, resembling that of many insects, and some larger animals in winter; and that its heart remains at rest, till by the heat of incubation it is roused into action. After the motions of the heart of the chick become visible, they may be rendered more lively or languid by a greater or less degree of warmth; may be made entirely to cease by cold, and be as quickly renewed again by heat *. Hence it follows, that though it be not certain when the heart begins first to move in nascent animals, yet the cause which sets it first a-going, and recommences its motions after being stopped, is heat, which, by rarifying and agitating with an intestine motion the particles of the fluids, enables them, by their *stimulus*, to excite the fibres of the heart into contraction; at the same time that it increases the sensibility of the nerves, which are benumbed by too great a degree of cold.

S E C T. V.

Of the motions of the alimentary canal, and bladder of urine.

HAVING thus accounted for the alternate contraction and relaxation of the heart; we come next to inquire into the cause of the other involuntary motions; and shall begin with those of the alimentary canal.

IN deglutition, the contraction of the muscles which draw up the *larynx*

* Harvey de generation. animal. exercitat. 17.

larynx and *os hyoides*, and so push the food into the dilated *pharynx*, is generally spontaneous, and owing to the irritation of the sensible membrane of the *fauces*, by the food passing that way. In like manner, no sooner is the aliment received into the *pharynx*, than this muscular tube contracts, and, embracing it closely, pushes it on to the *œsophagus*, whose nerves being irritated, and its fibres stretched by the food in its descent, each small portion of it contracts itself, and so transmits the aliment to the next, till at last it is pushed into the stomach.

THE aliments which are generally composed of parts fit to act as a gentle *stimulus* on the sensible parts of animals, are no sooner received into the stomach, than by its heat and motion, as well as the action of humours flowing into it, they begin to swell, and continue, during the whole time of their dissolution, to emit bubbles of elastic air: At the same time the cool air swallowed, every now and then, with the *saliva*, is quickly rarified by the heat of the stomach.

HERE then, we see, in the aliments, air, and humours, causes which may gently stimulate the nerves of the stomach, and stretch its fibres so as to excite them into contraction: and this agrees with Wepferus's observations, which shew that the contraction of the stomach never happens but in consequence of its preceding intumescence. "Motum ventriculi," says this author, "oculis observare licet in vivarum bestiarum anatome, et non semel vidi illum contrahi lentè versus stomachum, subsequente vomitu, aut versus pylorum contentis in duodenum explosis; nonnunquam gracilior et brevior reddebatur; mox iterum intumuit, subsequente rursus nova coarctatione, quæ vel versus pylorum vel stomachum progrediebatur *." And, in another place, "Quando vero circa medium se contraxerat (scil. ventriculus) motus lentè versus pylorum progrediebatur, illeque erigebatur liquorque subpallidus, nunc spumeus, nunc viscidus, quandoque sensim aliquando cum impetu protrudiebatur: constricto pyloro, rursus intumuit totus ventriculus, moxque rursus circa medium se constrinxerat, novusque liquor per pylorum

* Histor. cicut. aquat. p. 87.

“rum ejiciebatur *.” Thus we find the dilatation of the stomach always succeeded by its contraction, which expelling by the *pylorus* the distending cause, *i. e.* part of the air and aliments, the fibres of this organ are immediately relaxed, so as easily to give way to the distending force of the rarified air arising from the aliments, and thus to suffer a new dilatation; which, as before, is soon followed by a new contraction. Hence there appears a remarkable analogy between the causes of the alternate motions of the stomach and heart, even in those animals whose make as to these parts agrees with that of the human kind; but which is still stronger in granivorous birds, whose stomach more nearly resembles the heart in its structure, and in the force of its motions †.

If it be asked, why the stomach is not brought into a new contraction by the stimulating quality of its contents, before a new intumescence of it has happened? it is sufficient to answer, that, in order to excite a new contraction of the stomach, or hinder its yielding to the dilating force of the rarified air, the gentle *stimulus* of the aliment may require the additional irritation which the stretching of its fibres produces. Besides, as the convulsive contractions of the diaphragm which happen in consequence of an irritation of the left orifice of the stomach, do not follow one another very quickly, although the stimulating cause continues to operate, but after longer or shorter intervals, according as the irritation is weaker or stronger; so, in the same manner, after one contraction of the stomach is over, some time may be required for the stimulating cause to act, before a new contraction is produced.

THAT the ordinary vermicular motion of the stomach is chiefly owing to the stimulating quality of its contents, is confirmed by a variety of facts: thus, when any thing is received into the stomach, which strongly irritates or disagreeably affects its nerves, it is thrown into convulsive contractions, which are renewed, after short

* *Histor. cicut. aquat.* p. 177.

† The small rough stones which granivorous birds swallow, are not only useful to break and grind their food, but also to excite their less sensible stomachs into proper contractions; for these, on account of the hard skin which lines them, would be little affected by the dissolving aliments and rarified air, without the attrition of those stones.

short intervals, till the offending cause is either expelled or much weakened. On the other hand, *opium*, which renders our fibres and nerves less sensible of any irritation, has of all things the greatest power to quiet convulsive and irregular motions of the stomach. At the same time, the *nausea* and inclination to vomit, which is sometimes brought on by wind collected in the stomach, or by suddenly drinking too great a quantity of the mildest liquids, shews that the simple intumescence or distension of the stomach contributes to its contraction, independent of any particular *stimulus* affecting its nervous *papillæ*.

THE vermicular motion of the intestines is similar to that of the stomach, and produced by the same causes. Small quantities of rarified air and digested aliment are pushed from one portion of the intestinal tube into the next, and from this again into the succeeding one, and so on; *i. e.* the part dilated by the air and aliments acquires such a power of contraction, as to overcome the elasticity or contractile power of the contracted part next it. Whence should this happen? It does not seem to be owing to the more copious influx of arterial blood into the vessels of the distended segment, as Dr Stuart imagines *; since it has been shewn that this fluid does not immediately conduce to the contraction of a muscle †. There is no reason to believe, that, on account of any alternate compression of their nerves, the animal spirits are transmitted to the intestines in successive streams: nor would this, if supposed, answer the appearances; since the whole intestinal canal is not, like the heart, alternately contracted and relaxed, but, as to time, is irregular in the motions of its several parts. It remains therefore, that the stretching of the fibres of the inflated portion of the intestine, together with the *stimulus* of the bile, air, and digested aliments which it contains, is the cause of its subsequent contraction. And surely, if warm air impelled through the *vena cava*, or thoracic duct, into the heart of an animal newly killed, excites it into contraction, it may reasonably be supposed to have an equal effect on

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the

* Dissert. de motu muscul. cap. 12.

† Sect. i. No. 2. above.

the stomach and intestines, between whose motions and those of the heart there is a strong analogy.

WE have already seen from Wepferus, that, by every contraction of the stomach, some part of the more liquid aliment and rarified air is pushed through the *pylorus* into the *duodenum*, which, not finding a free passage through this intestine, on account of its valves and the natural contractility of its coats, will dilate that part of it next the stomach, and consequently excite it into contraction, by which its contents will be transmitted to the next portion of this gut, and so on through the whole tract of the *jejunum* and *ileum*, where the valves being larger, will, by stopping the progress of the chyle and rarified air, occasion greater dilatations, and consequently stronger succeeding contractions.

IT will further appear, that the peristaltic motion of the intestines, is owing to the digested aliment, bile, and rarified air, acting upon them as a *stimulus*, if we consider that purgatives, which act chiefly by irritating the inside of the intestines, greatly increase this motion: that in animals opened alive, the intestines are excited into stronger contractions, by pricking them with a sharp instrument, or applying any acrid liquid to them: that such things as render our nerves and fibres less sensible of any irritation, lessen or destroy the peristaltic motion of the bowels; thus Dr Kaau not only found the vermicular motion of the intestines extremely weakened and slow, in a dog to whom he had given six grains of *opium*, but that pricking their external surface with the point of a needle, did not sensibly increase it*; and, lastly, that when the bile from any cause becomes inert, or is hindered from flowing into the bowels, costiveness generally follows. Nay, that the *stimulus* of the bile is, in a particular manner, necessary to the right performance of the peristaltic motion, and that without it the intestines would not be able, sufficiently, to resist the distending power of the rarified air, seems probable from the remarkable inflation of the bowels in such as die of an inveterate jaundice; and from the case related by Dr Stuart, of a man who died of a wound.

* Impet. faciens, dictum Hippocrat. No. 435.

wound in the gall-bladder, who was not only incurably bound, but who had his intestines so much distended with air, that before the body was opened, a tympany was suspected *.

WHILE the chyle is taken up by the lacteal and absorbent veins of the small intestines, the grosser and less nutritious parts of our humours and aliments are transmitted from the *ileum* by the valve of the *colon* into the great guts, where they remain for some time without giving any disturbance; till, by the pressure of the diaphragm and abdominal muscles in respiration, together with the gentle contractions of the intestines themselves, they are pushed into the *rectum*, where, partly by their acrimony, but chiefly by their weight and bulk overstretching its fibres, they excite this gut into such contractions as bring on that strong desire of emptying it. When any acrid matter lodged in the *plicæ* of the *rectum* irritates the nervous *papillæ* of that part, as in a *tenesmus*, its muscular coat is forced into frequent and strong contractions, and there is almost a perpetual desire of going to stool. This is best cured by oily and mucilaginous clysters, with *opium*; which at the same time sheathe the acrimony and blunt the sense of pain.

THE bladder is a hollow muscle, which, wanting a proper antagonist, would always, if not hindered by some foreign cause, reduce itself to its smallest capacity, by means of that natural contraction owing to the force of the circulating fluids, the elasticity of its fibres, and the constant but gentle operation of the nervous influence upon them †. This contractile power of the bladder, whereby it reduces itself to its smallest size, is overcome by the urine gradually oozing into its cavity from the ureters; which at length, by overstretching its coats, excites the muscular fibres into strong contractions; but these being of themselves unable to open the *sphincter*, the diaphragm, abdominal muscles, and *levator ani*, assist them: however, after the *sphincter* is opened, the contractility of the bladder alone is sufficient to expel the urine.

THIS fluid, though sensibly acrid, does not, when accumulated

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in

* Philosoph. Transf. No. 414.

† Sect. i. No. 4.

in the bladder of a healthy person, occasion uneasiness so much by the irritation of the nerves of that organ, as by overstretching its fibres: but when the *mucus*, formed by nature to defend these nerves from the acrimony of the urine, is abraded, or when the inner coat of the bladder is inflamed or excoriated, no sooner does the urine begin to be collected, than, by fretting this very tender and sensible part, it occasions convulsive contractions of the muscular coat of the bladder, and also of the diaphragm and abdominal muscles; hence the patient is afflicted with a strong and almost constant desire of voiding his urine.

MARES and cows, after evacuating their urine, are observed, for some time, alternately to contract and relax their *urethra* and *sphincter vesicæ*; at first briskly, afterwards more weakly and with longer intervals between the contractions. These are solely owing to the irritation of the parts by the urine; and prove, that as the uneasy sensation begins to abate, so does the force and quickness of the motions.

S E C T. VI.

Of the motions of the blood-vessels, and of several other motions of the spontaneous kind.

THE most remarkable of the spontaneous motions which remain to be accounted for, are the alternate *systole* and *diastole* of the arteries; the less perceptible motion of the veins; the oscillatory contractions of the smaller vessels; the erection of the *penis*; the convulsive motions of the *musculi acceleratores urinæ* in coition; the motions of the Fallopian tubes, whereby they embrace the *ovaria*, and convey the *ovum* to the womb; the alternate action of the muscles of respiration; their convulsive motions in coughing and sneezing; and the contraction of the pupil and muscles of the internal ear, in order to adapt those organs to the degree of light and sound applied to them.

I. THE

1. THE *diastole* of the arteries, like the dilatation of the heart, is owing to the blood pushed into their cavity, with a considerable force, and their *systole* or succeeding contraction is effected chiefly by their elasticity, and partly by the proper contraction of their muscular coat, excited by the blood gently stimulating their internal surface, and at the same time stretching their fibres. That the *systole* of the arteries is not wholly owing to the elasticity, but partly also to the muscular contraction of their fibres, is generally acknowledged by physiologists; and that the blood alternately pushed into them and acting as a *stimulus*, excites this muscular contraction, the analogy of the heart, and other spontaneous motions, already explained, seems fully to evince.

2. BESIDES the alternate *diastole* and *systole* of the larger arteries, which, in a great measure, depend upon the projectile force of the heart, and the elasticity of their coats, there is a vibrating or oscillatory motion in the inferior orders of vessels, to which the direct force of the heart does scarcely reach, and where elasticity is not concerned. And as the food is conducted from the mouth through the whole course of the alimentary canal, by its exciting the muscular coat of this tube into contractions, as it passes along; so the motion of the fluids through the inferior orders of vessels and secretory tubes of the glands, to many of which the impulsive force of the heart seems not to extend, is chiefly carried on by the vibrating contractions of these vessels, excited by the gentle *stimulus* of the circulating fluids *.

3. As the smaller vessels, though destitute of any alternate pulsation depending upon the contraction of the heart, are nevertheless agitated with a kind of oscillatory motion; so it is reasonable to think, that the veins are not inactive canals, but are so affected by the *stimulus* of the circulating blood, as to have their muscular coat excited into alternate, but weak contractions, by means of which the
return

* See this point more fully illustrated in my *Physiological Essays*, edit. 3. essay 1. On the motion of the fluids in the very small vessels of animals.

return of this fluid to the heart is partly promoted. As a proof of this, the *vena cava* may be plainly seen to contract alternately in dying animals whose *thorax* is laid open *; though it is probable that the motions of this vein near the heart are more remarkable than elsewhere, on account of some kind of alternate depletion which it suffers, and of its coats being somewhat more muscular here than elsewhere. And does not the *vena cava* continue to palpitate longer than the heart in dying animals, because, after the circulation through this organ has ceased, the blood is still transmitted in small quantity from the arteries into the nascent veins, and consequently into the *cava*; which being therefore longer supplied with the cause exciting its motions, must continue them longer?

HENCE we see that the fluids are in some sense the cause of their own motion; since, without their stretching power and stimulating quality, the heart and arteries, however well fitted for muscular contraction, would remain unactive and at rest: and that as the contractile power of the solids is necessary to carry on the vital functions, so likewise is the action of the fluids upon the solids, in order to excite their muscular power into action. Hence also we may easily understand how it is that heat has so great an influence in promoting the circulation of the fluids, that insects and many other animals, after they are to all appearance dead, may be soon brought to life by it. Heat not only renders the heart and vascular system more sensible, but, by raising an intestine motion and brisk vibrations in the particles of the fluids, must necessarily communicate some degree of irritation to the solids; and thus bring them into alternate contractions.

4. THE erection of the *penis* has been generally ascribed to the contraction of the muscles called *erectores*; yet as their situation is such, that the veins of the *penis* can scarcely be affected by their action, and as an erection cannot be procured at pleasure, by strongly pressing it against the *os pubis*, several of the later authors seem justly to have rejected this opinion, but, as far as I know, without substituting any thing satisfactory in its place.

BUT

* Sect. xiv. No. 16. below.

BUT as the taste, nay even the sight or remembrance of grateful food, makes the *saliva* flow, in greater quantity than usual, into the mouth of a hungry person, by increasing the oscillatory motions of the salivary vessels [N^o 2.] ; so, why may not the *stimulus* of the semen in the *vesiculæ seminales*, or certain ideas, occasion a more than ordinary flow of blood through the small arteries of the *penis*, by increasing their vibrating contractions? If this really happens, the capillary red arteries, by the increased force of the fluids, will be enlarged, and the serous ones, at least many of them, will be rendered capable to admit red blood: those arteries which end in veins will transmit their fluids to those veins as usual, only with greater force, while such as terminate with open orifices in the cells of the *penis*, will, through their dilated mouths, pour forth not only a serous or lymphatic fluid, as usual, but the red blood itself; which not being fast enough carried off by the absorbent veins, whose orifices are not enlarged proportionally with those of the arteries, must fill and distend the cells, and consequently produce an erection. As soon as the causes exciting an extraordinary oscillatory motion in the small vessels of the *penis* cease, the tumor begins to subside, because the fluids are then poured in less quantity into the cells.

THIS account of the erection of the *penis* seems to be more agreeable to the laws of the animal œconomy, than that proposed by Duvernoi *, who supposes that the small veins of the *penis* may be so straitened, by a contraction of the nervous filaments surrounding them, as in a great measure to prevent the return of their blood †. Vieussens, who considered the nerves which in some places appear to surround the blood-vessels, as so many small cords capable of constricting them more at one time than another, and consequently of producing remarkable changes in the

* Act. Petropolit. tom. 2. p. 379. 383. 384.

† Supposing the nerves could, like cords, straiten the veins of the *penis*, yet an erection of this member would not be the consequence; for tho' a ligature made on the *penis* near the *pubis* makes it swell, yet this swelling is quite different from a true erection. See Albinus's *Annotationes academicæ*, lib. 1. cap. 18.

the circulation, deduces the paleness of the face in some passions, and its redness in others, from the nerves straitening the carotid arteries in the former case, and the jugular veins in the latter *. However, none of these authors have shewn how the nerves can be so drawn as, like a ligature, to straiten the vessels which they encompass. Nor indeed can this be easily conceived. There is no example to be found of any motion or action in the animal body being performed by the contraction of the nerves, whose office is not to be drawn or rendered more tense at one time than another, but to supply the muscular fibres every where through the body, with that influence or power which seems to be immediately necessary to their contraction: but, by the increased oscillatory motion of the small vessels, which we have assigned as the cause of the erection of the *penis*, we daily observe a variety of sudden and surprising changes produced in the circulation. To this is to be ascribed the profuse secretion of pale limpid urine, to which hysterical people are so liable; as also the tears from the lachrymal vessels, in people affected with great joy or grief. And the blushing, or redness and glowing warmth of the face, which attends a sense of shame, is not owing to the constriction of the temporal veins by means of the nervous filaments from the *portio dura*, which surround them, but to an increased oscillatory motion of the small vessels of the face, which in most people, more or less, accompanies a consciousness of shame: for as the rosy colour, and sudden warmth, which are the necessary consequences of the increased motion of the blood in the smaller vessels, ill agree with the stagnation of this fluid, as arising from any compression of the temporal veins; so their being often diffused over the neck and breast, clearly shews that they cannot proceed from this cause. Why this affection of the mind should produce such a change in the circulation of the blood in those parts rather than any other, we do not pretend to say. Sufficient it is, that from experience we know that the several parts of the body are variously affected by the different passions of the mind.

AFTER what has been said, it will be easy to account for the
stiffening

* Neurograph. lib. 3. cap. 4. p. 182.

stiffening of the nipple, and the swelling of the comb and rattles of a Turkey cock, which are similar to the erection of the *penis*, and which, as M. de Haller has justly observed, are certainly not owing to the contraction of any muscle hindering the return of the blood by their veins *. For why may not the passion of anger occasion an inflation of the above-mentioned animal's comb and rattles, as well as the sense of shame does a flushing of the face? and may not titillation increase the motion of the fluids in the small vessels of the nipple, in the same manner as in the *penis*?

THE unusual sensation of heat in the face, which attends blushing, and is so quickly raised, may enable us to account for the sudden complaints of heat and cold, and other symptoms of a like nature common to hysterical people; for if an affection of the mind can raise an uncommon heat in the face, by increasing the action of the nervous influence upon its vessels, by which they are affected with an uncommon vibratory motion; why may not the same thing happen in other parts of the body, in consequence of an irritation of their small vessels, or of some commotion of the nervous system? and is it not probable, that the sudden sensations of cold, which people subject to nervous complaints often feel in various parts of their bodies, are owing to the stagnation or slower motion of the fluids in the smaller vessels of these parts, occasioned by the diminution or suspension of their oscillatory motion?

BUT to return from this digression; whether the erection of the *penis* is effected in the manner above explained, or by the contraction of certain muscles compressing its veins, it is nevertheless, like the other spontaneous actions, owing to an irritation, *viz.* the stimulus communicated to the nervous *papillæ* of the *vesiculæ seminales* and testicles by the *semen*; since, in proportion to the abundance or defect of this, erections are, *cæteris paribus*, more or less frequent, stronger or weaker. 'Tis true, that lascivious thoughts, titillation, and other causes, often produce erections of the *penis*; but even their power of doing this is in a great measure owing to the presence of the *semen*.

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* Prim. lin. physiolog. No. 800.

AN erection of the *penis* sometimes happens from the bladder being full of urine, at least is increased thereby; because the urine, by stretching and stimulating the coats of that organ, affects the nerves and vessels of the *penis*, with which the bladder is so nearly connected.

5. IN time of coition, as soon as the *semen* is squeezed into the beginning or bulbous part of the *urethra*, the *musculi acceleratores urinae* which surround it are brought into convulsive contractions, which are repeated till the seed is intirely expelled; and that these convulsive contractions are owing to the *semen* acting as a *stimulus* upon this part of the *urethra*, cannot well be denied; since their number and force are greater or less, according as this liquor is more or less in quantity, or more or less prepared.

6. BY the titillation of the *rugæ* of the *vagina*, not only is the *uterus* affected, but the *tubæ Fallopianæ* becoming rigid, have a kind of erection; at which time their fimbriated extremities are turned towards the *ovaria*: nor do they change this situation till the *ovum* has been received into their cavity, through which it is pressed forward to the *uterus*, by the contraction of the muscular coat of those tubes; which, from the analogy of the other involuntary motions already explained, we may easily imagine, is excited by the *ovum* as it passes along their internal surface; so that every small portion of them, will, by its contraction, transmit the *ovum* to the succeeding one, till at last it drops into the cavity of the womb; in the same manner as the food, in a horizontal posture, is conveyed through the *æsophagus* into the stomach.

THAT the convulsive motions of the muscles of respiration in coughing and sneezing, and of the diaphragm in the hiccup, are owing to an irritation of the sensible membrane of the nose, windpipe, and inferior part of the gullet, is too evident to need any particular proof; and the less, as these motions will be illustrated in the sequel of this essay. Nor is it less true, that the motions of the pupil and muscles of the internal ear, are owing to light and sound acting as *stimuli*

stimuli on those organs: but as these motions, whereby the eye and ear are accommodated to different degrees of light and found, are more intricate and less generally understood, I shall treat of them particularly in the following section. And as the alternate motion of the organs of respiration differs from the other spontaneous motions already explained, in being so far under the power of the will, that we can accelerate, retard, or for a considerable time put a stop to it, and is a subject upon which a great deal may be said, I shall also treat of it in a section by itself; where its cause will be shewn to be analogous to that of the other spontaneous motions.

S E C T. VII.

Of the motions of the pupil and muscles of the internal ear.

AS the degrees of light to which the eye is exposed, and the splendor of the objects presented to it are various, had the pupil been of a determinate size, incapable of enlargement or diminution, that organ would have been adapted only to contemplate objects in one particular degree of light; every thing brighter than this would have dazzled it, while a fainter light would not have affected it sufficiently. Further, as the rays of light coming from near objects, are much more divergent than those from remote ones, had the pupil been incapable of variation as to its extent, the eye would have been ill fitted for seeing distinctly at different distances; since such objects alone are seen distinctly whose images are painted upon the middle and most sensible parts of the *retina*.

To prevent these inconveniencies, and that the eye might be capable of receiving properly the impressions of objects in a great variety of lights and distances, that membrane called the *uvea* or *iris*, which encompasses the pupil, is furnished with a double set of muscular fibres, by whose contraction or relaxation the diameter of this passage can be augmented or diminished. One plane of these fibres is circular, and immediately surrounds the circumference of the pupil: it may properly be called the *sphincter pupille*,

since, by its contraction, the pupil is lessened. The other is composed of a number of radiated fibres, which take their rise from the great circumference of the *uvea*, where it is attached to the *circulus albus*, or union of the *cornea* and *sclerotica*, and are inserted into the orbicular muscle above mentioned, all round the circle of the pupil, as the spokes of a wheel are into its nave. This plane of muscular fibres acts as antagonist to the orbicular one, and may be called the *laxator* or *dilatator pupillæ*.

THE circular plane of fibres is so thin and delicate, that several authors have doubted of its existence; but in admitting it, we are not only supported by the authority of the best anatomists*, but by reason and analogy; since the equable and regular contraction of the pupil cannot well be conceived without supposing some such mechanism; and since we find the other passages in the body which are endued with a power of contracting themselves furnished with *sphincter* muscles.

FURTHER, although the circular and longitudinal fibres of the *uvea* are so delicate, that their muscular structure cannot be clearly demonstrated by anatomists, yet they may be sufficient for the performance of the various motions of the pupil; for we daily meet with insects whose agility shew them to be furnished with muscles, although those instruments of motion in them are too minute to be discovered by any dissector.

THE figure of the pupil, as well as its degrees of contraction and dilatation, are different in different animals. In man the pupil is round; in horses, cows, &c. it is oblong and transverse; and in cats in the day-time, it forms a narrow chink perpendicular to the horizon, but in the dark acquires nearly a circular figure, and becomes almost as large as the *cornea*. If in cats the pupil had been circular as in man, it could not well have admitted of so great degrees of dilatation and contraction, which yet are necessary to an animal which must seek its prey in the night-time; at least, when most contracted, its edges must have been furled, and their thickness much increased, by being folded together in so small a space.

* Winslow Anatom. sect. 10. No. 220. Ruyfch. Thesaur. anatom. 2. tab. 1. fig. 5. lit. c.

space. Moreover, it is observable, that those animals which have the pupil of an oblong or oval shape, are capable of seeing in a much fainter light than man.

GALEN, who did not allow of any motion to the pupil, except when one of the eyes is shut, ascribed the dilatation of the pupil of the open eye to its having the spirits which used to be bestowed on both eyes now determined into it alone. Achillinus, who lived in the beginning of the sixteenth century, makes particular mention of the motion of the pupil from different degrees of light *; which however was so little attended to, that its first discovery has been generally ascribed to Father Paul of Venice, who lived about a hundred years after him †. However, neither Father Paul, nor Fabricius ab Aquapendente his cotemporary, seem to have known how these motions are performed. Nor ought it to appear strange, if, before the muscular structure of the *uvea* was known, physiologists were much in the dark with regard to the motions of the pupil.

THE natural state of the pupil is that of dilatation; for since the longitudinal fibres of the *iris* are much more conspicuous and stronger than the circular plane, they must, by their natural contraction ‡, keep the pupil always dilated, unless the latter are excited into action by some cause.

WHAT this cause is, could be no difficult matter to discover: for, as in a *syncope*, apoplexy, or at the moment of death, when the eye is insensible to external objects, the pupil is always much dilated; as in the shade it is large, and always the more so, the greater the darkness; while in a bright light it is contracted almost to a point; it follows, that the contraction of this passage is owing to the action of light on the eye as a sensible organ, and that its dilatation is owing to the superior contractile power of the longitudinal fibres of the *uvea*, when the eye is left to itself, and not affected by any external cause.

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* Morgagni. *Adversar. anatom.* i. p. 34.

† Fabricius ab Aquapendente de oculo, part. 3. cap. 6. *Plemp. Ophthalmograph.* lib. i. cap. 9.; et *Douglas Bibliograph. anatom.* p. 228.

‡ Sect. i. No. 3. and 4. above.

THE pupil is contracted more or less in proportion to the quantity of light admitted into the eye, not on account of any immediate action of this subtile fluid on the fibres of the *iris*, as some have imagined *, but in consequence of its affecting the tender *retina* with an uneasy sensation. Hence whatever intercepts the rays of light so as to prevent their affecting the *retina*, or whatever renders this membrane insensible to their action, occasions a preternatural dilatation of the pupil. Thus, in a cataract, where the crystalline humour being considerably opaque, intercepts a great part of the luminous rays in their way to the *retina*, the pupil is not so much contracted as in a sound eye. In a confirmed *gutta serena*, or perfect insensibility of the *retina*, the orbicular muscle of the pupil loses its power of contraction, inasmuch that this passage remains equally wide in the sunshine as in the shade. If the action of light on the circular fibres of the *iris* were the cause of their contraction, this would not happen; since the nerves of this membrane, as they have no connection with the optic nerve, ought to remain equally fit for actuating its orbicular muscle, and equally sensible of the *stimulus* of light, when the *retina* is thus diseased, as in a sound eye. But if it shall be alledged, that in a *gutta serena*, the nerves of the *uvea* become paralytic, and that the immobility of the pupil is owing to this, and not to the insensibility of the diseased *retina*; I answer, that a plain experiment shews the contrary: Thus when one eye is quite lost by a confirmed *amaurosis*, if the sound one be covered or kept shut, the pupil of the diseased eye remains in every degree of light immoveable, and of the same size; but if the sound eye is exposed to the sun-beams, the pupil of the other, which shewed no motion before, will be observed to contract. This contraction can only arise from the sympathy between the two pupils; and shews, that when the sound eye is covered, the defect of motion in the morbid eye is not owing to the nerves of the *uvea* being paralytic, but to the want of a cause increasing their action upon the orbicular muscle of the pupil.

FURTHER

* Histoire de l'Acad. des sciences 1704, edit. 8vo, p. 18.; et Memoires, p. 360.

FURTHER, if the contraction of the pupil proceeds from light acting as a *stimulus* on the fibres of the *iris*, why does it not excite its longitudinal fibres equally into action as its circular ones?

WHEN the head of a living cat is put under water, its pupil, which was contracted before, is immediately dilated, though exposed to the sun-beams. if the contraction of the *sphincter pupillæ* arose from the action of light on its fibres, this appearance would hardly admit of a solution; since it does not appear that the rays of light should act with much less force upon the *iris* of an animal under water, than in the open air: but, on supposition that the contraction of the pupil is owing to the *stimulus* of light affecting the *retina*, it is easily accounted for. The rays of light passing from air into the eye, through the *cornea*, suffer a considerable refraction on account of its greater density; by which means they are made to approach one another so as, by the refractions of the crystalline and vitreous humours, they may be collected in a point on the *retina*. But when the head of an animal is under water, the rays of light suffer little or no refraction in passing through the *cornea* and aqueous humour, because their density scarce differs from that of water: hence they will not, as in the former case, be made to approach one another, nor will they have their *focus* in the *retina*, but a great way behind it; this membrane, therefore, will be weakly affected by them, and consequently the pupil must be dilated. In water there is a general and faint light diffused over a great part of the *retina*; in air all this light is collected, and acting on a much smaller space, is more vivid and striking.

MR MERY has given a different solution of this appearance, and his account is this. Under water, the animal is hindered from breathing, but the motion of the spirits, to which he ascribes the contraction of the pupil, depends on the circulation of the blood, and this again on respiration; therefore, notwithstanding the usual action of light on the *iris*, the pupil in an animal under water must be relaxed merely on account of the interruption of respiration*. It is most certain, that in a *syncope*, when the vital motions

* Memoires de l'Acad. des sciences 1704. edit. 8vo, p. 353.

motions cease, the pupil is dilated in air as well as in water, because the *retina* loses its sensibility; but a cat plunged into water does not become immediately insensible, nor does the motion of its heart cease with that of respiration; and if a man can restrain breathing near a minute, without losing any of his senses, this animal, which bears the air-pump so long, must continue sensible and lively under water for a much longer time: add to this, that the pupil of the cat is observed to be much dilated immediately after immersion; whereas, according to Mr Mery's principle, it should become gradually wider as the animal languishes more and more.

SINCE the optic nerves and those of the *uvea* arise from different parts of the brain, and have no communication with each other in their course to the eye, it seems evident, that light affecting the *retina* cannot excite the *sphincter* of the pupil into contraction, by any immediate mechanical change which it produces, either in the muscle itself, or in the nerves which actuate it; but the uneasy sensation occasioned in the *retina* by the admission of too much light into the eye, may so affect the sentient principle, which is present and ready to act where-ever the nerves have their origin, as to excite it to contract the orbicular muscle of the *uvea*, in order to lessen the pupil, and exclude the offending cause. While the eye remains in the same degree of light, and directed towards the same object, the pupil remains invariably of an equal size, as the same cause continues uniformly to excite the mind to determine the nervous influence in the same degree into its *sphincter* muscle: but no sooner does the light become fainter, than the sentient principle, being less affected, ceases to contract this muscle, and allows the curtain of the pupil to be opened by the natural action of its longitudinal fibres, by which means more light is admitted to the eye.

WHEN the eye is suddenly removed from a faint into a bright light, a considerable dazzling, with an uneasy sensation, is perceived; and though in smaller changes this is less perceptible, yet it may be sufficient to excite the mind to contract the pupil so far as may be necessary in such cases to defend the tender *retina*.

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IF it be asked, why the orbicular muscle of the *uvea* is rather contracted than its longitudinal fibres, upon the admission of light into the eye? the answer is, that the contraction of the latter would not tend to remove the uneasy sensation, but to increase it: and such is the original constitution of our frame, that the mind or sentient principle is, in consequence of certain uneasy sensations, instantly determined to produce such motions or changes in the body as tend to remove or lessen them.

WHEN a candle is placed before the eyes, if one of them is covered with the hand; or any other opaque body, the pupil of the other will be observed immediately to become wider. Now, as the muscles of the *uvea* of the one eye have no manner of connection with those of the other, either by means of nerves or blood-vessels, unless it be that the former are derived from different parts of the same brain, and the latter from the *aorta*, this *consent* in their motions must be inexplicable upon mechanical principles alone: for if the action of light on the eye is the cause of the contraction of the pupil, why should not the pupil of the open eye remain equally contracted when the same degree of light continues to act upon it; or why should it be affected by the relaxation of the other pupil, with which it has no immediate connection, while the mechanical cause of its own contraction continues to act with undiminished force?

BUT if we allow the contraction of the pupil to be owing to a sentient active principle, which, in proportion as it is more or less affected by the uneasy sensation arising from the action of light on the *retina*, contracts the pupil in a greater or less degree; then, when one eye is shut, its *retina* being no more exposed to the light, and consequently the sentient principle being no longer excited to contract the orbicular muscle of the *uvea*, its pupil must be widened by the natural contraction of the stronger longitudinal fibres of that membrane: but as the mind has, from the time of birth, been always accustomed to contract the pupils of both eyes at the same time, the one pupil can no more be relaxed without the other being partly relaxed, than one eye can be directed to the nose,

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while the other is turned from it: for how much soever the motions of certain muscles are owing to the immediate energy of the mind, yet it is undeniable, that, by constant habit, we soon lose the power of moving them, except in a particular way: and as this is true of the eyes, whose motions are of the voluntary kind, and may be performed or restrained at pleasure, it cannot well be denied to take place in such muscles whose action is from the beginning independent on the will.

As in the above-mentioned experiment the pupil of the open eye is considerably relaxed, on account of the consent of its motions with the pupil of the other, so it is not to be doubted, but that the pupil of the covered eye is less enlarged than it would be, if no light was admitted into the open one: thus in a *gutta serena*, the blind eye has its pupil sensibly contracted, when the sound one is exposed to a bright light, *i. e.* the diseased pupil follows in some degree the motions of the sound one, and, by the action of light upon it, is hindered from being so much relaxed as it would otherwise be. It is however probable, if a perfect *amaurosis* were to continue long in one eye without affecting the other, that this consent between the pupils, as to their motions, would gradually become less sensible, till at last the pupil of the diseased eye would cease to be lessened almost in any degree by the action of light on the sound one.

WHEN a candle is so placed as to shine full upon one eye, without any of its rays having access to the other, the pupil exposed to the light is observed to be somewhat less than the other; though neither of them is so much contracted as if both eyes were equally affected by the light. Hence it follows, that notwithstanding there is a considerable uniformity between the pupils of both eyes as to their motions, yet they do not exactly keep pace, but the pupil immediately exposed to the greatest light is most contracted. And this serves further to shew, that when one eye is covered, the pupil of the open one becomes wider, from the *consent* between its motions and those of the darkened one; and not, as some may perhaps imagine, because the mind receiving only the impression of light acting upon one eye, and therefore being less affected than when both eyes are open,

open, makes a less effort to exclude the light, by contracting the pupil. The reader will, however, perceive, that in both ways of explaining the above *phenomenon*, the necessity of deducing these motions of the pupil from the mind, is equally acknowledged.

IF it shall be alledged, that the contraction of the pupil is not owing to the action of light on the *retina*, but on the choroid coat, and that the *uvea* being a continuation of this membrane, and having its nerves from the same source, may be supposed to have a sympathy with it: Without entering into the question, whether it is the *retina* or the choroid that receives the impression of objects, and feels the *stimulus* of light; I answer, that as the agreement just now observed between the two pupils as to their motions, cannot arise from any mechanical consent between them, but must be owing to some common principle in the brain; so it is reasonable to imagine, that the contraction of the orbicular muscle of the *uvea*, in consequence of light being admitted into the eye, proceeds from the same cause, and not from any connection between the choroid and *uvea*: for supposing it did, why should its circular fibres rather than its longitudinal ones be contracted; since, as the latter are nearer to, and more immediately derived from the choroid than the former, one would be apt to think their sympathy with it should be the more remarkable?

HAVING, as we hope, given a clear and consistent account of the motions of the pupil, so far as they are owing to different degrees of light applied to the eye; it may not perhaps be improper, briefly to point out the opinions of some learned authors of the greatest character, with respect to this matter, different from mine.

M. MERY, not being able to observe any circular fibres in the *iris*, whose contraction would account for the constriction of the pupil, endeavours to shew that this is owing to the inflation and elongation of its longitudinal fibres by the animal spirits being more copiously derived into them; and that the dilatation of the pupil is owing solely to the spring or elasticity of these fibres, whereby they become shorter when left to themselves *. But there is no

* Memoires de l'Acad. des sciences 1704, edit. 8vo, p. 352.

instance in the human body, of any muscle becoming longer by a more copious derivation of the nervous influence into it; the constant effect of this is, to swell, harden, and shorten the muscle at the same time. Nor indeed would it be easy to conceive (allowing a singular structure in the longitudinal fibres of the *iris*) how they could be so lengthened by the influx of animal spirits, as to reduce the pupil almost to a point, without, at the same time, being so inflated as to make a remarkable difference in the thickness of this membrane.

M. DE LA HIRE imagines, that as a bright light, by disagreeably affecting the bottom of the eye, excites us to contract the pupil; so, in the dark, we do our utmost to dilate it, that we may see more distinctly *; that is, its dilatation is owing to an effort of the will, occasioning a stronger action than usual of the influence of the nerves on the longitudinal fibres of the *iris*. To prove this, he observes, that cats in a luminous place, when they do not seem to be taking notice of any thing around them, have their pupils almost quite shut, but that, as soon as any extraordinary object presents itself, so as to draw their attention, they immediately, and at once, open them considerably †. If the fact be true, as I presume it is, then cats must be allowed to have a power of dilating, and probably also of contracting their pupil at pleasure, when the quantity of light applied to their eyes remains the same; which however is not the case with men, in whom the wideness of the pupil in a *syncope*, apoplexy, and confirmed *gutta serena*, shews, that, in order to dilate the pupil to its largest size, no effort of the mind is necessary, but only the superior contractile power of the longitudinal fibres of the *uvea*, when its circular muscle is not excited into action by the *stimulus* of light on the *retina*. Nor have I ever been able to observe that the pupil is narrower in a light room, when one does not attend to any thing around him, and becomes wider as soon as he looks at any object within a proper distance of the eye, so as to see it distinctly.

THE same learned author, in consequence I suppose of his above-mentioned

* Memoires de l'Acad. des sciences 1709. edit. 8vo, p. 121.

† Ibid. p. 121, 122.

mentioned notion of the voluntary dilatation of the pupil, also alleges, that, in a bright light, when we look attentively at an object in order to see its small parts, the pupil is not so much contracted as it would be by the action of this degree of light alone, did we make no such effort to see any thing distinctly *. This, however, is so far from being true, that it will appear, from experiments to be mentioned below, that in the strongest light the pupil is less contracted when we make no effort to see any thing distinctly, than when we look with great attention to a near object, so as to observe its minute parts.

AN illustrious author, equally doubtful of the existence of the circular, and of the action of the longitudinal fibres of the *uvea*, deduces the contraction of the pupil from the *stimulus* of light affecting the *iris*, and occasioning a greater flux of humours into its fine pellucid vessels, by which means they are extended in length, the *iris* is rendered broader, and consequently the pupil narrower. The dilatation of the pupil, he ascribes to the aqueous humour pressing its edges outward, when the powers contracting it, and consequently resisting the pressure of this fluid, are weakened. But, if the contraction of the pupil were owing to the elongation of the vessels of the *iris*, from the humours moving with greater force through them, then, in animals newly dead, warm water injected into the carotid artery should make the pupil contract sensibly. Further, the *iris* should become thicker and its vessels should swell; since the greater force of the fluids moving in them must increase their transverse diameter, as well as their length: thus when the *penis* is erected by the effusion of blood into its cells, it becomes thicker, as well as longer. Lastly, As this theory supposes the contraction of the pupil to proceed from the action of light as a *stimulus* upon the sensible vessels of the *iris*, it may be looked upon as insufficient, from what has been offered above, to shew that it is to the action of light on the *retina*, and not on the *iris*, that the contraction of the pupil is owing †.

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* Memoires de l'Acad. des sciences 1709, edit. 8vo, p. 121. 122.

† It has been urged as a proof of that author's opinion concerning the contraction of the pupil, that when the jugular veins were tied in a cat, the pupil became narrower (a); but this experiment

(a) See Relationes de libris novis Gotting. 1752.

WITH respect to his account of the dilatation of the pupil, we may likewise observe, that as the watery humour, like all other fluids, must necessarily press the parts of the *iris* as much inwards toward the pupil as outwards towards the *cornea*, it can have no effect in widening the pupil. Unless therefore that it can be shewn, contrary to the principles of hydrostatics, that the parts of the aqueous humour are not in *equilibrio* among themselves, this opinion must necessarily fall to the ground. But further, if the dilatation of the pupil were not owing to the natural contractility of the longitudinal fibres of the *uvea*, but merely to the pressure of the aqueous humour upon its edges, when the power contracting it ceases to act; the pupil should, contrary to experience, continue to grow wider for some time after death, because the vessels and fibres of the *iris*, becoming then much more flaccid, must be less able to resist the supposed pressure of the aqueous humour: but if the enlargement of the pupil is owing to the natural contraction of the longitudinal fibres of the *iris*, as has been above explained, then it will appear why the pupil does not become wider, but rather narrower after death; because these fibres which retract its edges gradually lose their contractile power, and are somewhat lengthened *.

M. WINSLOW is, I believe, the first who observed that the pupil becomes less after death. In some bodies he found it of a moderate size, in others a good deal more contracted, but never much dilated, as we know happens in a *gutta serena*, *syncope*, apoplexy, &c.† The pupil, however, not only thus becomes narrower after death, but also sometimes before it.

THUS in a boy of 5 years of age, who had been for some days comatose,

experiment only seems to shew that the vessels of the *iris* as well as the vessels of the *conjunctiva* and indeed of the whole face, are much distended when the return of the blood from them to the heart is prevented. This distension of the vessels of the *iris* must have rendered the pupil somewhat narrower: but I should imagine that this diminution of the pupil is as different from its contraction occasioned by light, as the swelling of the *penis*, in consequence of a ligature made upon it, is different from a true erection.

* See some farther observations on this subject in my *Physiological Essays*, edit. 2. p. 144, &c.

† *Memoires de l'Acad. des sciences* 1721. edit. 8vo, p. 416.

comatose, the pupil first became remarkably wide, and was not sensibly affected by a lighted candle brought very near the *cornea*: about 15 hours after this, looking into his eye, I observed, with some surprize, the pupil not larger than in a sound eye in a moderate light. At this time having endeavoured to rouse him, by holding spirit of *sal ammoniac*. to his nostrils, and making him swallow some cinnamon water with *sp. volat. oleos.* the pupil was thereby suddenly dilated, and became as wide as it had been the evening before. After half an hour, he was seized with a greater degree of *stupor*, and his pupil became again remarkably less as above, and so remained equally in all degrees of light; but upon applying a second time the spirit of *sal ammon.* to his nose, it was quickly enlarged, so as to occupy two thirds of the *cornea*. This experiment I repeated four times in the space of two days, and always with the same success. During most of this time, the pulse was strong and full. When his head was opened after death, we found immediately below the *corpus callosum* about two ounces of water.

FROM this case it seems probable, that the dilatation of the pupil soon after the coming on of the *coma*, was owing to the compression of the *thalami nervorum opticorum*, by the water collected in the brain, which rendered the *retina* insensible of the *stimulus* of light. Soon after, the origin of the nerves of the *uvea* beginning to be compressed by the growing collection of lymph, the longitudinal fibres of this membrane lost their power of contraction, and became flaccid, almost as in dead bodies; whence the edges of the pupil were less retracted. The volatile spirits applied to the olfactory nerves, by giving a shock to the whole brain and nervous system, in some degree opened the obstructed nerves of the *uvea*, so as to allow their influence to be derived into its fibres, the necessary consequence of which was the dilatation of the pupil. But as soon as the effect of this *stimulus* was over, the influence of their nerves being again intercepted, the longitudinal fibres of the *uvea* were relaxed, and therefore the pupil was less dilated. As these different states of the pupil were more remarkable in the left than in the right eye, it is probable, that one side of the

the medullary substance of the brain was somewhat more compressed than the other. Lastly, since, after the pupil was enlarged by the *stimulus* of the volatile spirits, the eye still remained wholly insensible to the action of light, it is reasonable to think, that the pressure upon the origin of the optic nerves was greater than upon that of the nerves of the *uvea*.

2. THE motions of the pupil are not only necessary to adapt the eye to different degrees of light, but also to the distinct vision of objects at different distances. Thus if a book which one can easily read at the distance of two feet, is gradually brought nearer the eye till the letters can be no longer distinguished, the pupil will be observed to become narrower in proportion as the book approaches the eye. Again, if one looks first to a candle two or three feet distant, and immediately after to the point of a quill, or any such object, within five or six inches of the eye, and nearly in the same direction with the candle, the pupil will be sensibly contracted: now, as the same quantity of light from the candle shines upon the eye in these two cases, the greater contraction of the pupil in the latter case cannot be owing to the light more strongly affecting the *retina*, but to an effort of the mind to see the object more distinctly. This is further confirmed by the following experiment: Let one with his back to the light, first look to an object of a lively colour at the distance of three or four feet from his eyes, and afterwards to a dark one at the distance of five or six inches, and the pupil will be observed to become sensibly narrower when he looks at the near object, altho' the quantity of light reflected from it be less. Therefore the learned Dr Jurin seems to be mistaken when he says, that in a faint light the pupil is so far from contracting in order to distinct vision, that there is rather a necessity of dilating it to take in more light*; for in a faint light the pupil is always observed to be wider when we look at an object three or four feet from the eye, than when it is placed so near it as to be seen indistinctly.

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* Essay on distinct and indistinct vision, at the end of Smith's Optics, p. 145.

THE necessity of the contraction of the pupil when we look at near objects, in order to render vision more distinct, is easily understood; for as in near objects the divergency of the rays is greater than in distant ones, and as those rays only serve for distinct vision which do not diverge much from the *axis* of each pencil, the pupil must be contracted, in order that the useless or disturbing ones may be excluded. The contraction therefore of the pupil in viewing near objects, is not solely owing to the spissitude of the rays reflected from them, as Plempius, and others, have thought *, but chiefly to an effort of the will in order to distinct vision: in like manner, when we look at remote objects, the pupil not only becomes wider, because the rays are thinner, and consequently their light fainter, but also because its sphincter muscle, on account of the weakness of the light, is not near so much contracted in order to distinct vision, as when we view objects which are placed at a less distance from the eye than the limit of distinct vision.

IN viewing distant objects, the pupil is not widened by any effort of the mind, but its size is chiefly determined by the quantity of light applied to the eye, which, as it is, *ceteris paribus*, fainter in distant than in near objects, must occasion a small degree of dilatation in the pupil, when we contemplate the former: but in looking at any thing nearer the eye than that distance at which we see distinctly, and with the greatest ease, the contraction of the pupil is principally owing to a voluntary exertion of the mind's power in order to render vision more distinct; and but in a very small degree, to the stronger and more vivid light, which the object, on account of its vicinity, reflects upon the eye †.

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* Plemp. Ophthalmogr. lib. 3 cap. 8.

† Although the learned Dr Porterfield has unadvertently quoted this paragraph in such a manner (particularly by leaving out these words, *on account of its vicinity*) as to expose it to censure (a); yet its truth is proved by the following easy experiment. If, while one looks steadfastly at a pin at the distance of three feet from the eye, another pin be placed in the same right line in the *axis* of vision, within three or four inches of the eye, the pupil will not be sensibly lessened, although more rays enter the eye from this near pin than from the more distant one; but as soon as the person views the near pin, the pupil will be evidently contracted in order that it may be seen more distinctly.

(a) See his Treatise on the eye, vol. 1. p. 171.

M. MAITRE JAN has erroneously asserted, that the pupil is lessened when we look at distant objects, and enlarged when we behold near ones; and endeavours to shew the usefulness of these motions to distinct vision *.

THE ingenious author of the *Essais de physique* seems to have fallen into an error likewise inconsistent with the laws of vision, when he observes, that the images of objects in the bottom of the eye are greater or less, as the pupil is more or less dilated; and hence accounts for the sun's appearing larger when he first appears above the horizon, than at mid-day, when his greater splendor makes the pupil to be more contracted †. But if this were so, objects ought to appear always largest in the faintest light; which we do not find confirmed by experience. Further, since the image of any object is formed by the union of the pencils of rays that flow from each point of the object in correspondent points of the *retina*, it appears, that its magnitude depends on no other circumstances, but the real magnitude of the object, and its real distance from the eye ‡. The different size of the pupil may, agreeably to what has been observed above, affect the lustre and accuracy of the several points of the image, but cannot alter their distance, because the *axis* of the several pencils, and their angles of inclination, continue the same, whatever is the bulk of the pupil. Any one may satisfy himself at once, by an easy experiment, that the contraction and dilatation of the pupil cannot alter the bulk of the image of any object formed on the bottom of the eye; for the picture which is made by a common lens on a sheet of paper, will be found to shrink or swell, when greater or lesser concentric portions of the lens are covered.

As we have already seen, that the pupil cannot be so much contracted by the action of light alone on the eye, as when, along with this, there is an effort of the will to see a near object more distinctly; so the pupil cannot, by any effort in order to distinct vision, be

* *Maladies de l'œil*, chap. 8 & 21.

† *L'Anatomie d'Heister*, avec *Essais de physique*, edit. 2. p. 703.

‡ It is not affirmed, that the apparent magnitude of objects depend on the circumstances here mentioned alone; for we are well aware, that experience, the known distance of objects, and other things, have great influence in this matter.

be as much lessened in a faint light as in a bright one. Thus, if one with his back to the windows of a room, brings a small printed book so near his eyes, that he cannot, without straining, distinguish the letters; upon turning his face quickly to the light, he will be able to read with little difficulty*; because, by the action of the stronger light on the *retina*, the pupil is immediately lessened; and therefore its power, to prevent the dissipation of the rays, and consequently indistinct vision, is increased. Hence neither the single effort of the mind to avoid indistinct vision, nor a vivid light alone, can contract the pupil to its least size, that is, not so much as when both these causes of its contraction are united.

In infants, but more especially in such as are newly born, the pupil is observed to be wider in proportion to the breadth of the *iris*, than in grown people, when the eyes of both are exposed to the same degree of light; 1. because in *fætuses* and new-born children, the *cornea* being thicker, less transparent, flatter, and not sufficiently stretched, on account of the small quantity of aqueous humour †, the *retina* is less affected by the rays of light, which are neither freely transmitted to, nor properly collected upon it ‡; and 2. because it is probable that they want, in a great measure, the faculty of

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contracting

* See Dr Jurin's essay on distinct and indistinct vision.

† Memoires de l'Acad. des sciences 1727, edit. 8vo, p. 348. 349. and 350.

‡ It has been objected to this, that if in infants vision is indistinct, on account of the *cornea* being thicker, flatter, and less transparent in them than in adults, their pupil ought not to be wider, but more contracted, in order to render vision more distinct (a). But as the above-mentioned differences between the eyes of new born infants and those of grown persons do really take place, the greater wideness of the pupil in the former than in the latter shews the fallacy of this reasoning; and at the same time demonstrates, that the eyes being full and plump, or much distended with the aqueous and other humours, cannot be the cause of the wideness of the pupil in new-born children, as the learned Dr Porterfield maintains (b): On the contrary, as Petit has observed, the *cornea* which is a good deal flat at birth, becomes afterwards more prominent, by the increase of the quantity of the watery humour. I should only add, that when objects are not clearly seen, because the rays of light are not transmitted to the *retina* on account of an opacity of the crystalline, watery humour, or *cornea*, the pupil is not contracted to remedy this defect, as it is, when objects are seen indistinctly, on account of their being placed either too near or too far from the eye.

(a) See Dr Porterfield's Treatise on the eye, vol. 1. p. 170, and 171.

(b) Porterfield's Treatise on the eye, vol. 1. p. 163.

contracting the pupil, in order to the more distinct vision of near objects, which seems, like the uniform motions of the eyes, to be partly acquired by habit. The causes, therefore, to which the contraction of the pupil are owing, being weaker in new-born infants than in adults, it is no wonder that this passage appears more dilated in the former than in the latter.

IN old people, the pupil becomes less moveable, because the *retina* grows less sensible of the *stimulus* of light, and the muscular fibres of the *iris* lose in part their contractile power: further, in old age, the *cornea*, on account of the decrease of the aqueous humour, loses its brilliancy; whence the *retina* will be less affected by light, and consequently the pupil would become wider, were it not that this is more than compensated by the diminished elasticity of the longitudinal fibres of the *uvea*, and by the eye becoming less, in consequence of a diminution of the quantity of its humours, whence the diameter of the *circulus albus*, to which the *uvea* is attached, being lessened, the aperture of the pupil must become narrower.

THE motions of the pupil from light differ from those which are performed in order to render vision less indistinct; since the former are owing to an uneasy sensation affecting the *retina*; whereas the latter arise from an act of volition, or effort of the will in order to the more distinct vision of objects at certain distances. The former are of the same kind with the contraction of the diaphragm in the hiccup and in vomiting, of the muscles of respiration in sneezing, and of the *acceleratores urine* in expelling the *semen*. The latter agree with the motions of the crystalline, by which the eye is adapted to see distinctly at different distances, and with the uniform motions of the eyes in looking at objects. The first are always necessary and independent of the will; but the second are plainly voluntary, and can be restrained if we please, though they are often not attended with consciousness of volition.

IN looking at near objects, the pupil is lessened, at the same time that the crystalline humour is brought forward towards the *cornea* by the contraction of the ciliary processes; but when we contemplate more distant ones, the contraction of the ciliary processes and
orbicular

orbicular muscle of the *uvea* ceasing, the crystalline returns to its natural situation, and the pupil to that size to which it is fixed by the quantity of light applied to the eye. These motions, though both voluntary, yet come to be so connected by habit, that we cannot perform them separately; nay, as often as we direct our eyes to any near object, the motion of the crystalline and contraction of the pupil naturally go along, and are performed in such a degree, as is most proper to procure distinct vision at that particular distance; nor can we separate these three motions; although, as they are all voluntary, they may be restrained or performed at pleasure.

THE pupil differs from the *anus*, neck of the bladder, and other passages guarded by *sphincters*, in being always dilated when nothing adventitious acts on the eye, while the latter, left to themselves, are constantly contracted; the reason of which is, that the natural and equable contraction of the longitudinal fibres of the *uvea*, which serve to dilate the pupil, is strongest, while the contrary holds true of the other passages now mentioned, whose *sphincter* muscles have either no proper antagonists, or such as are much weaker than themselves. Further, after death, when these *sphincters* are relaxed, and consequently their passages rendered more patent, the pupil becomes sensibly less, because the longitudinal fibres of the *iris*, to whose contraction its dilatation was owing, lose their contractile power, grow flabby and longer. Hence we see, that at death the eye-lids remain partly open, for the same reason that the pupil is lessened after it.

IF, from any cause, the longitudinal fibres of the *uvea* are rendered quite paralytic, while its circular muscle retains its usual power, the pupil will, by the natural contraction of the latter, be at all times very much contracted, so that in a bright light it will become little less than in the shade. If the circular, as well as longitudinal fibres of the *iris*, are intirely paralytic, the pupil will lose its whole motion, and be much in the same state that it is in some time after death, *i. e.* it will be generally as much contracted as the pupil of a sound eye is in a moderate degree of light. If the muscular fibres of the *iris* are not wholly deprived of their contractile power, but
much

much weakened, the pupil in a dark place will have its edges a little retracted by the action of the longitudinal fibres, and in a bright light will be somewhat lessened by the contraction of the circular ones; but these motions will be much more inconsiderable than in a sound eye.

IN all the cases above mentioned, the disease called by Hippocrates, and others among the ancients, *ἡμεραλωπία*, will happen, *i. e.* the patient will see in the day-time, but in the twilight and night-season, he will not be able to distinguish objects. An instance of this I had lately in a young man of about 27 years of age, who had served for some time in the navy, where he had been exposed to much fatigue and cold: his eyes appeared sound; nor could I observe that they differed from the eyes of other people, excepting that the pupil had very little motion: it remained always pretty narrow, and was neither remarkably contracted by light, nor dilated in the dark. This person saw well enough in the day-time, especially if the weather was serene; but in the twilight, or in an obscure place, was so far from distinguishing objects, that he could scarcely find his way: as his pupil had some motion, it is probable, that the fibres of the *iris* were not quite paralytic, though much weakened: his seeing in the day-time shewed, that the *retina* was either wholly, or in a good measure, sound; and that his blindness in a faint light was owing to the pupil's not being dilated so as to admit a sufficient quantity of rays into the eye.

IF, after an inflammation of the *iris*, a rigidity is left on its circular or longitudinal fibres, the pupil will be deprived, either wholly, or in a great measure, of its usual motions; and may be either too much contracted, or greatly dilated: if the former, the patient will only see well by day; if the latter, the eye will not be able to bear a bright light, and therefore the patient will see best in the shade and by candle-light; *i. e.* he will labour under the disease called *νυκταλωπία*.

ALTHOUGH in an *amaurosis* the pupil is generally much dilated, yet this is not always the case; for as often as the fibres of the *uvea* are intirely deprived of their contractile powers, the pupil appears

appears as much, or rather more, contracted than in a sound eye. Of this I saw an instance, not long ago, in a woman who was almost totally blind of both eyes, where there was no opacity in the *cornea* or crystalline humour, but only a want of motion in both pupils. The pupil of the right eye was immoveable, and always as much contracted as it should have been in a moderate light; the other was dilated, as is usual in a *gutta serena*, but when exposed to the light seemed to contract a very little. By means of this eye she discerned light from darkness, but could not distinguish objects: with the other eye she saw nothing.

IN this patient both eyes seem to have been affected with a *gutta serena*; in the right eye the *retina* was not only insensible, but the muscular fibres of the *uvea* must have been quite paralytic, whence the pupil remained always moderately contracted, as Winslow has commonly observed it some time after death *. In the left eye the *retina*, though considerably diseased, was not wholly destitute of feeling, and the fibres of the *uvea* seem to have been sound and possessed of their usual contractile power.

Of the motions of the muscles of the internal ear.

AS, without the motions of the pupil, the eye would have been ill contrived for vision in different degrees of light, and at different distances; so the ear would have been unfit for hearing distinctly a diversity of sounds, were not some of its parts capable of various degrees of tension. A musical chord, of a determinate length and tension, can only vibrate harmonically with one particular sound; if therefore there was no mechanism, by means of which the membranes of the *tympanum* and *feneſtra ovalis* could be more or less stretched or relaxed, they could only be harmonically affected by one sound; which, therefore, alone would be heard distinctly, and all others more or less confusedly. To prevent this inconveniency, the *malleus* is furnished with three muscles, and the *stapes*

* Mém. de l'Acad. des sciences 1721, edit. 8vo, p. 416.

stapes with one; by the various contractions of the former, the membrane of the *tympanum*, and by means of the latter, the membrane of the *fenestra ovalis*, is rendered more or less tense, and so accommodated to almost all possible sounds.

IT may well appear wonderful, how the ears should be so exactly adapted, by the various contractions of these muscles, to such a vast variety of sounds; but with what exquisite skill and amazing wisdom is every thing in the animal frame adjusted! As the *stimulus* of light upon the *retina*, and the sensation of indistinctness in near objects, excite the mind to contract the pupil, so the less distinct tremor of different sounds, affecting the auditory nerves, is the cause of the subsequent contractions of the muscles of the internal ear; for no sooner does the mind perceive the first indistinct noise of any sound, but it instantly contracts some of the above muscles, so as most nicely to adapt to it the membranes of the *tympanum* and *fenestra ovalis*: if the sound be acute, these membranes are just as much stretched as is necessary for their vibrating harmonically with it; if it be flat, they are duly relaxed: and thus, by a most simple mechanism, the ear is rendered sensible of the smallest variations of sound or difference of notes in music. As infants seem, by habit, to acquire a faculty, or at least a greater dexterity of adjusting their eyes, by the motions of the pupil and crystalline humour, to the various distances of objects; so it is not improbable, that they may at first hear less distinctly, till, by degrees, they come to acquire a power of readily accommodating their ears more exactly to different sounds. And is not the want of an EAR (as it is usually called) owing to a deficiency of this power*? While that exquisite discernment of musical sounds, which many possess, shews, that they can adjust their ears to different notes with the greatest accuracy.

THAT the motions of the muscles of the internal ear proceed from the mechanical action of sound or vibrating air on their fibres, the analogy of the motions of the pupil would seem to contradict. And, if

* This may arise either from a less degree of sensibility in the auditory nerves, or of agility in the muscles of the ear, or from both.

if this were the case, why should not all the muscles of the *malleus* be equally contracted by the same sound? And why should those which serve to stretch the membrane of the drum be excited into motion by acute sounds, while the muscle which relaxes it is only brought into action by grave ones? As brute animals, upon the first perception of any noise, turn their external ears towards the place from whence it comes; so, at the same time, they adapt their internal ear to it; the first of these motions cannot be denied to flow from their sentient principle actuated by the sound; why then should we doubt that the latter proceeds from the same cause?

THE motions of the muscles of the internal ear, in consequence of various sounds, are not only unattended with any consciousness of volition, but are altogether involuntary; for we cannot move them except when sound strikes the ear, nor hinder them to act when it does.

S E C T. VIII.

Of the alternate motions of respiration.

RESPIRATION is that action whereby a certain quantity of air is alternately received into the lungs and expelled out of them: it consists of inspiration and expiration. Inspiration, or the reception of air into the lungs, is owing to the contraction of the intercostal muscles and diaphragm, whereby the cavity of the *thorax* is both lengthened and widened; for as the lungs, together with the heart, &c. perfectly fill the cavity of the breast, and as their exterior surface is every where contiguous to the *pleura* and *diaphragm**,

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* Some authors contend, that the lungs are not contiguous to the *pleura*, and that, in the space between them, there is contained what they call internal air, the use of which in respiration they endeavour to shew; but the arguments, upon which this opinion is founded, do not appear to be conclusive, and which I think I could easily shew, were it not foreign to our present design; nor is the authority of the illustrious Morgagni sufficient to support that doctrine (a), since the best anatomist may be deceived in making a single experiment, and since we have the repeated experiments of some of the other great anatomists against him; nay, in every dead body, where the integuments and intercostal muscles are carefully taken off, the lungs may be seen contiguous to the *pleura*.

(a) *Adversar. anat.* 5. *animadver.* 33. p. 46.

it necessarily follows, that when the *diaphragm*, by its contraction, descends, and the ribs, by the action of the intercostal muscles, are raised, the lungs must follow them, and consequently the external air must rush in by the *glottis*, to fill the vacuity that would otherwise happen in the cavity of the chest.

INSPIRATION being thus performed, the inspiratory muscles are relaxed, upon which the ribs by the renitency of their elastic cartilages return to their former situation, and the diaphragm, by the reaction of the stretched *peritoneum*, *pericardium*, and abdominal muscles, is pushed up into the *thorax*; whose cavity being therefore diminished, the air contained in it must be expelled by the *glottis* *.

IN order to account then for the alternate motions of respiration, it is only necessary to shew, why the intercostal muscles and diaphragm are alternately contracted and relaxed; since their contraction occasions inspiration, and their relaxation allows the elastic force of the cartilages of the ribs, &c. to produce expiration †. But as Mr Bremond has, in the Memoires of the academy of sciences for 1739, favoured us with some experiments, which he thinks clearly prove, that the lungs are not passive in respiration, but are endued with a power of dilating and contracting themselves independent of the motions of the *thorax*; it will be necessary, before we proceed any further, to shew, how far this ingenious gentleman has been deceived; and that the lungs have really no proper alternate motion of their own, but follow the motions of the chest.

OF the experiments related by Mr Bremond, the following are the most remarkable.

1. AFTER a wound made in the *thorax* of a dog, the lungs, instead of collapsing, continued to be contracted and dilated alternately, but their motions were asynchronous to those of the *thorax*; for when the

* While the cavity of the *thorax* is lessened by the causes here mentioned, the muscular fibres of the *bronchia*, by their contractile power, contribute to the expulsion of the air from the lungs.

† If any one doubts of both rows of intercostal muscles conspiring to pull up the ribs, he need only compare what Winslow has said on this head (Anat. sect. 3. No 1057. 1058. 1059.) with the course and direction of these muscles in a fresh subject; or, if that cannot be had, with the elegant figures of them published by Dr Hoadly at the end of his lectures on respiration.

the ribs were depressed, a small portion of the lungs was thrust out at the wound with a considerable force *.

2. AFTER making an opening in the breast, and breaking four or five of the ribs, the lungs continued for some time to be alternately expanded and contracted; and issued, always, out at the wound, when the *thorax* collapsed †.

3. AFTER three ligatures were made upon the *trachea* of a dog, and the *thorax* was opened, the alternate motions of the lungs were observed to go on very briskly; but they appeared to be dilated, and issued out at the wound when the chest was contracted, and *vice versa* ‡.

HOWEVER difficult it may be thought, at first sight, to account for the appearances in these experiments, yet it is easy to shew, that the lungs cannot be endued with a power of expanding themselves, independent of the dilatation of the *thorax*, as Mr Bremond seems to think, after Platerus, Sennertus, and others, who in this followed the Arabian physicians.

As the lungs are composed of an infinite number of vesicles and cellular interstices §, which are partly made up of elastic contractile fibres, it is easy to perceive that when distended with air, they must endeavour to reduce themselves to their former bulk; but, supposing the vesicles of the lungs empty and collapsed, by what mechanism can they expand themselves, or where are the antagonist muscles that can overcome the natural contraction of their fibres? All the hollow muscles of animals are continually endeavouring to contract, nor can they be dilated by any mechanism of their own: thus the bladder of urine, which may be compared to

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* Memoires de l'Acad. des sciences 1739, edit. 8vo, p. 463. and 465.

† Ibid. p. 464. and 465.

‡ Memoires de l'Acad. des sciences, p. 468.

§ The reader may see, that our reasoning here is of equal force, whether the lungs are supposed entirely cellular, with Helvetius (a), or partly vesicular partly cellular, with Winslow (b).

(a) Memoires de l'Acad. des sciences 1717, edit. 8vo, p. 30. 31. &c.

(b) Anatomie, sect. ix. No. 105. 106. 107.

a single vesicle of the lungs, spontaneously contracts itself, when the urine is evacuated, and would for ever remain in this state, were it not dilated by the urine flowing anew into it. In the same manner the stomach and intestines, which are hollow muscles, have nothing in their structure by which they can expand themselves: they are ever endeavouring to arrive at their least capacity, and are only prevented by the aliment, air, and other fluids contained in them. It follows, therefore, that the pulmonary vesicles and cells which are composed of elastic contractile fibres, cannot be dilated by any power or action of their own.

BUT further, if Mr Bremond's experiments prove any thing, the lungs have not only a power of inspiring air by the *trachea*, but, although this fluid is denied its usual access by the *glottis*, can expand themselves alternately, notwithstanding the pressure of the atmosphere upon their external surface resisting such expansion; which is much the same as if one should affirm, that a bladder perfectly empty, with a tight ligature about its neck, could, by its own proper power, swell and overcome the pressure of the external air.

HAVING shewn that the lungs cannot possibly be endued with a power of expanding themselves, it remains, that we now inquire, to what causes their apparent motions, in Mr Bremond's experiments, were owing.

IN the two first experiments above mentioned, when the cavity of the *thorax* was enlarged by the action of the inspiratory muscles, a small portion of air would be received by the *glottis* into the lungs of the wounded side: but as this could bear no greater proportion to the air entering by the wound, than the aperture of the *glottis* did to this opening, the ribs would at that time recede from the lungs, which therefore would seem to subside. When the *thorax* collapsed, the air contained betwixt the ribs and the surface of the lungs escaping by the wound, the lungs would soon become contiguous to the ribs, and even part of them would be pushed out at the opening, not only on account of the convulsive contraction of the *thorax* squeezing the lungs much more than in ordinary

nary expiration, but partly from the small quantity of air received by the *glottis* during the former inspiration, and not yet wholly expelled, which being rarified by the heat of the lungs, must considerably increase their bulk.

THE apparent contraction therefore of the lungs, when the dogs endeavoured to inspire, as remarked by Mr Bremond in his experiments, is in my opinion to be ascribed entirely to the enlargement of the *thorax*, which at this time rises from the anterior surface of the lungs; while they, following the diaphragm now strongly drawn down towards the *abdomen*, really recede from the *sternum*. And if, during expiration, upon the ribs being depressed, and the diaphragm being forcibly thrust up into the cavity of the breast, by the strong convulsion of the abdominal muscles, the lungs really appeared to swell, and some portion of them was even forced out at the aperture made in the *thorax*; yet this ought not to be attributed to an expansion of the lungs happening at this time, but rather to the subsiding of the ribs and the protrusion of the diaphragm into the chest, whose cavity being therefore much diminished, the lungs (in some degree inflated) not only fill it, but being pressed on all sides, are thrust out at the wound, where there is least resistance; as soft clay, leaven, or such like substances when squeezed in the hand, are forced through the interstices between the fingers.

BUT further, while the *thorax* is wounded only on one side, the lobes of the other side continue to follow the motions of the chest as usual, and to be alternately inflated with air rushing into it by the *glottis**: when therefore, during expiration, the air is forcibly expelled out of these lobes, although the greatest share of it escapes by the *glottis*; yet, since fluids press equally every way, some part must enter the large branches of the *trachea* which belong to the lobes of the wounded side, and consequently dilate it; and this the more remarkably, as the animal in straining to cry contracts its *glottis* more or less, and so renders the egress of the air through it more difficult; hence, during expiration, this part of the lungs will appear to swell considerably, and some portion of this part will be

* Highmore Disquisit. anatom. p. 188.

be pushed out at the opening in the *thorax*, not only as its cavity is at this time much lessened, but as these lobes are in some degree inflated: it is plain however, that even this inflation is owing to the contraction of the *thorax* forcing the air out of the lobes of the sound side.

THIS is further confirmed by an observation of Dr Houston, who observes, that when the dog cried, his lungs burst out at the wound, but when he was silent, they retired within the *thorax* *; and even by an experiment of Mr Bremond's own, in which although, after making an opening into one side of the *thorax*, and breaking three or four ribs, the lungs were pushed out at the wound, every time the breast was contracted; yet, as soon as the anterior part of the *thorax* and *sternum* were raised, and both sides of the breast thus laid open, the lungs instantly collapsed, and remained so without any motion, notwithstanding the heart, ribs, and diaphragm continued their alternate motions for some time †.

HIGHMORE, who mentions his having made numerous experiments upon dogs, in order to satisfy himself concerning the manner in which respiration is performed, observes, that when both sides of the *thorax* had large wounds made in them at once, the lungs always collapsed‡: nay, there is not one of the many experiments related by Mr Bremond himself, in which the lungs were pushed out of the *thorax*, when both sides of it were opened.

IT must be owned indeed, that, in two experiments of the illustrious Baron Van Swieten, the lungs did not collapse, after both sides of the *thorax* were perforated, but were protruded through the apertures, during expiration ||. However, it is to be observed, that the wounds seem to have been but small; since he afterwards adds, that when the opening in each side of the *thorax* parallel to the ribs was half an inch or more in length, the animals quickly died. Further, while the animals lay quiet, the lungs kept within the cavity of the breast, and were only thrust out of the wounds, when, from pain, they

* Philosophical Transactions abridged, vol. 9. p. 141.

† Memoires d' l'Acad. des sciences, 1739, edit. 8vo, p. 464.

‡ Disquisit. anatom. p. 188.

|| Commet. in Boerhaav. Aphor tom. 1. p. 271.

they made great efforts; and since in straining, or any violent motion of our muscles, we always contract the *glottis*, and retain the inspired air in the *thorax*, it is probable, that the eruption of the lungs through the wounds, in expiration, was owing to the air being retained in them while the *thorax* collapsed. Lastly, That illustrious author observed, that during inspiration the orifices of the wounds became less patent, by the ribs being drawn nearer one another, so that a less quantity of air would enter by them, and a greater proportion by the *glottis*: hence the lungs did not collapse, as always happened in Highmore's experiments, when very large openings were made at the same time in both sides of the *thorax*; but when a hollow tube, whose aperture greatly exceeded that of the *glottis*, was introduced into the wound of each side, the lungs instantly subsided, respiration ceased, the voice failed, and the dog seemed to die*.

WITH regard to the appearances in experiment 3d above mentioned, it ought to be observed, that the lungs in a natural state, equally fill the cavity of the *thorax* in inspiration and expiration; if therefore we suppose the lungs to have been in a middle state of distention, when Mr Bremond made his ligatures on the *trachea*, their bulk must have been considerably less than the cavity of the *thorax* when most enlarged, but at the same time greater than this cavity when the chest is violently contracted: so that we ought not to be surpris'd, if, upon the elevation of the ribs and depression of the diaphragm in inspiration, the lungs appeared to subside, and no longer filled the *thorax*; or if, upon the constriction of the breast in expiration, they seemed to swell, and part of them was pushed out at the wound.

BUT though we suppose the *trachea* to have been tied during inspiration, yet since the ingenious author found, notwithstanding the ligatures, that after the death of the dog, a small quantity of air blown forcibly through the *trachea* passed into the lungs, it is not improbable that the diaphragm and ribs strongly squeezing the lungs in expiration, had forced out part of the air contained in them through

* Commentar. in Aphorism. Boerhaav. tom. I. p. 271.

through the wind-pipe, so as to have reduced them to a moderate state of expansion.

LASTLY, if we should suppose the ligatures to have been made even about the end of expiration; yet the air remaining in the lungs being raised by their heat, would soon increase their bulk to such a degree, as that, when the *thorax* was much contracted, part of them must necessarily be thrust out at the wound. Perhaps also, during the enlargement of the *thorax*, a small quantity of air might, notwithstanding the ligatures, be received by the *trachea* into the lung of the sound side, which, upon expiration, would be chiefly forced into the lung of the wounded side; since its egress by the *glottis* would be difficult, on account of the ligatures on the *trachea*.

THAT the apparent motions of the lungs in the above experiments of Mr Bremond, were, in fact, chiefly owing to the motions of the *thorax*, particularly to the alternate depression and ascent of the diaphragm, an experiment related by the author himself must convince us; for he informs us, that the *abdomen* of a living dog being laid open, and a hole made in the fleshy part of the diaphragm on the left side, the lung of that side instantly collapsed, and remained without any motion, although the *thorax* continued to be alternately dilated and contracted for a considerable time*. Now, if the lungs were endued with a power of expanding and contracting themselves, why did the lung of that side, where the diaphragm was perforated, immediately collapse and cease to move? Besides, does not this experiment shew, that the apparent motions of the lungs, and their egress by the wounds, were chiefly owing to their being alternately pushed up into the cavity of the *thorax*, when the diaphragm was relaxed? That the ascent of this muscle into the chest, by the convulsive contraction of the abdominal muscles, especially when the dogs attempt to cry, contributes much more to the apparent swelling of the lungs and their issuing out of the *thorax*, during expiration, than the falling of the ribs, is evident from another experiment of Mr Bremond, who observed these motions of the lungs to continue, although, with his hands, he forcibly hindered the ribs of the wounded

* Memoires de l'Acad. des sciences 1739, edit. 8vo, p. 471.

wounded side from moving, and consequently from pressing the lungs alternately *.

M. BREMOND always observed, that when the ribs were elevated, the lungs appeared red, but became paler when the cavity of the breast was diminished †. The lungs being no longer pressed while the *thorax* was enlarged, admitted the blood from the right ventricle of the heart more readily into all their vessels; but being strongly compressed by the ascent of the diaphragm and depression of the ribs, this blood must in part be expelled; when of course they become paler.

WHAT has been said above, may serve also to shew how far M. Herissant is mistaken, when, from some experiments of a like nature with those of M. Bremond, he concludes, that the apparent alternate dilatation and contraction of the lungs in animals whose *thorax* is laid open, is owing, solely, to the blood pushed into the pulmonary artery by the right ventricle of the heart, which, by dilating and unfolding all its branches and ramifications, must make the whole substance of the lungs swell, and cause the air to rush into their vesicles by the *glottis* ‡. If this account of the matter were just, the alternate swelling and falling of the lungs would be very inconsiderable, and ought to correspond with the contraction and dilatation of the heart; which however is not the case. Besides, it ought not to be observed at all, when the passage of the air into the vesicles of the lungs is obstructed by ligatures made on the *trachea*, contrary to what happened in M. Bremond's experiments.

THUS much being premised, to shew that the lungs have not properly any inherent power by which they can alternately contract and dilate themselves, but that, in ordinary and healthful respiration, they follow the motions of the *thorax*, we proceed next to inquire, by what power or mechanism inspiration and expiration alternately succeed each other, or why the intercostal muscles and

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* Memoires de l'Acad. des sciences 1739, p. 482.

† Memoires de l'Acad. des sciences 1739. p. 483.

‡ Histoire de l'Acad. des sciences 1743, edit. 8vo, p. 103.

diaphragm are contracted and relaxed by turns, as long as life remains.

THE learned Boerhaave, in order to account for the alternate motions of the *thorax* in respiration, supposes, that, at the end of inspiration, the blood is transmitted in smaller quantity to the left ventricle of the heart, since the pulmonary vessels must be considerably compressed by the lungs, which, at that time are greatly distended with air: hence he conceives, that not only less blood will be distributed to the intercostal muscles and diaphragm, but, also, that the influence of their nerves must be weakened; as the secretion of the spirits in the *cerebellum* must be diminished, when a smaller quantity of blood is pushed into its vessels by the heart: the causes, therefore, which are supposed to contract the inspiratory muscles, being weakened at the end of inspiration, these muscles will be overcome by the natural refilition of the elastic cartilages of the ribs, together with the re-action of the abdominal muscles, &c. *i. e.* expiration must necessarily follow; but no sooner does the blood, by the motion of the lungs in expiration, flow in a more plentiful stream to the left ventricle of the heart, than the causes actuating the inspiratory muscles begin to increase; whence these muscles are contracted a-new; *i. e.* inspiration is produced, to which, for the reasons above mentioned, expiration must necessarily succeed; and in this way the alternate motions of the chest in respiration are carried on through life*. This theory, it must be owned, is ingenious, and has an air of simplicity which further recommends it; but if strictly examined, I doubt it will be found insufficient to account for the alternate motions of the *thorax*, and the other appearances of respiration.

1. FOR, if at the end of inspiration the intercostal muscles and diaphragm were relaxed, on account of the smaller quantity of blood and spirits then distributed to them, why should not the heart, which also receives its nerves from the *cerebellum*, be affected in the same way? If the secretion of vital spirits were diminished, on account of the flowing of less blood from the lungs to the left ventricle of the

* Boerhaave Institut. med. § 619. & 620.

the heart at the end of inspiration, how could the pulse be then equally strong as at the end of expiration? which however is the case, as far as our sense of feeling can determine.

2. ALTHOUGH we should grant, that at the end of inspiration the blood flows in a smaller stream to the left ventricle of the heart, yet it will not follow, that the *cerebellum* will cease to supply the vital organs with spirits sufficient for their alternate motions; since we know, that after both the carotid arteries have been tied in a dog, the motions of the heart and respiration went on in the ordinary way, while in the mean time the *cerebrum* and *cerebellum* were deprived of more than one half of the blood usually bestowed upon them *.

3. AFTER the lungs have been kept for some considerable time in a collapsed state by an effort of the will, the inspiratory muscles are no sooner left to themselves, than immediately they contract, and produce a new inspiration; which, however, could not happen if Boerhaave's theory was true; since, in this case, the blood must flow with more than ordinary difficulty through the pulmonary vessels, and consequently the causes actuating the inspiratory muscles must be considerably weakened †.

4. THE insufficiency of this account of respiration is further shewn by the experiments of Highmore and Bremond, who observe, that after both sides of the *thorax* were laid open, the diaphragm and intercostal muscles continued their alternate contractions for a long time, although the lungs were collapsed and motionless ‡. Here we find the motions of inspiration and expiration continued in the *thorax*, while the lungs remained in the same state, and when there

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* Van Swieten Comment. in Boerhaav. Aphor. vol. 1. p. 266.

† This argument must conclude with the greater force, since even Boerhaave himself allows, that the blood passes less freely through the lungs when they are collapsed than when they are inflated. “Pulmone per aërem distento, sanguini pulso ex cordis dextro thalamo, “latiora vasa arteriosa, et venosa, minus resistunt, transitum expediunt; faciunt ut omnis ille “rapiatur eo quam celerrime ventriculum sinistrum versus: collapsus idem vix per arteriam “pulmonalem impleri potest liquore impulsio; inflatus per vasa aërifera, facile sanguiferorum “impletionem patitur.” Boerh. Institut. med. § 200. No. 2.

‡ Highmore Disquisit. anat. p. 185.; et Memoires de l'Acad. des sciences 1739, edit. 8vo, p. 464. 467. 468.

was no cause which could make the blood flow alternately through the pulmonary vessels with greater or less ease, nor consequently render the inspiratory muscles alternately paralytic, through a defect of blood and spirits.

5. LASTLY, The various appearances of respiration, in the air-pump, diseases of the head, *asthma*, and the symptoms in melancholy people, are not to be accounted for from Boerhaave's theory, and are alone sufficient to overturn it: but of this more afterwards.

THE late ingenious Dr Martine, aware of the difficulties attending Boerhaave's account of respiration, has proposed the alternate compression of the phrenic nerves at the end of inspiration, as the cause which, at that time, renders the diaphragm paralytic, and consequently produces expiration: he seems indeed to doubt, whether the alternate pressure of the inflated lungs on the posterior part of the *pleura*, can, at the end of inspiration, intercept the influence of the nerves belonging to the intercostal muscles which lie behind it: but as the phrenic nerves run between the lungs and *pericardium*, and are only covered by the thin *mediastinum*, he thinks, that, at the end of inspiration, when the lungs are much distended with rarified air, these nerves must suffer such a remarkable compression, as to render the diaphragm to which they are distributed paralytic*. But this account of the motions of respiration, however ingenious, will be found perhaps still defective, and even less satisfactory than that of the learned Boerhaave.

1. BY Martine's theory we are not informed how the intercostal muscles are alternately contracted, as well as the diaphragm. It is not probable, that the nerves of the intercostal muscles can suffer any more compression than usual at the end of inspiration; nor would it serve the author's purpose to suppose they do, since he seems to agree with those who are of opinion, that the two orders of intercostal muscles are antagonists to one another, and consequently are contracted at different times.

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* Edinburgh Medical Essays, vol. 1. art. 12.

2. SINCE the author allows, that during inspiration the nerves become rather freer from compression, why should not the diaphragm remain contracted, and consequently the lungs continue in their most expanded state? He says indeed, that the inspired air, rarified by the heat of the breast, and not finding an exit free or wide enough by the *glottis*, will at this time press more upon the vesicles and membranes of the lungs; which pressure must be communicated to the phrenic nerves running along the *pericardium*, and which are covered only by the *mediastinum*.

BUT surely the rarification of the air at the end of inspiration, is not so remarkable or sudden, but that it can issue fast enough out by the aperture of the *glottis*, to preserve it in *equilibrio* with the external air: besides, as inspiration does not immediately follow expiration, but after a short pause, ought not the air contained in the lungs at the end of expiration to be rarified, and so produce the same effect, by its pressure upon the vesicles of the lungs and phrenic nerves, as at the end of inspiration? The author, it is true, seems to have been aware of this; and therefore supposes, that, at the end of expiration, the contractile fibres and membranes of the lungs will, by their re-action, prevent, in some measure, the inflating air from pressing with its whole force on the phrenic nerves. But will not this hold equally true at the end of inspiration? and will not the elastic fibres and membranes of the lungs re-act with much greater force when they are considerably stretched by inspiration, than when so much relaxed at the end of expiration? As the lungs, therefore, both in expiration and inspiration, equally fill the cavity of the *thorax*, the compression which the phrenic nerves suffer from them, will be pretty much the same at the end of expiration, as in a state of full inspiration; and consequently the difference of this pressure cannot account for the alternate motions of the diaphragm to which they belong.

3. IF, at the end of inspiration, the phrenic nerves suffered such compression from the inflated lungs, as to render the diaphragm paralytic, how comes it to pass, that, after a full inspiration, we can, by an effort of the will, keep this muscle, for a considerable time, in a strong

strong state of contraction, and thus hinder expiration from following inspiration?

4. FURTHER, in one of M. Bremond's experiments, where the lungs remained collapsed and without motion, and consequently where the phrenic nerves must have been exposed to an equal degree of compression, the intercostal muscles and diaphragm continued to be alternately contracted and relaxed for above a quarter of an hour*.

5. THAT the motions of respiration vary according to the quantity or quality of the blood thrown into the pulmonary artery, to the free or difficult passage it meets with in the vessels of the lungs, and to the heat or coldness, rarity or density of the air, are circumstances which cannot be explained by this theory.

6. LASTLY, N^o. 2. 3. 4. and 5. of the arguments mentioned in sect. 2. to shew that the alternate motions of the heart cannot be owing to the compression of the nerves, are here of equal force; for the soft and spongy lungs are less capable, even when inflated, of compressing the nerves, than the firmer arteries and auricles of the heart.

THE various opinions of other writers, I shall not stay to enumerate, much less undertake to examine, but proceed to give an account of the motions of the *thorax*, which it is hoped will tend equally to explain the appearances observed in respiration, whether the lungs and other instruments concerned in it be in a natural or diseased state.

1. DURING inspiration and expiration, the blood finds an easy passage through the vessels of the lungs, as by their alternate inflation and contraction, it is pressed forward to the left ventricle of the heart. After inspiration is completed, it begins to flow with more difficulty; and at the end of expiration (if inspiration does not soon succeed) its motion is still less free. After expiration, therefore, the blood, on account of its difficult passage thro' the pulmonary vessels, is partly accumulated in them, and, by stretching their sensible fibres and membranes, acts as a *stimulus* upon the pulmonic nerves,

* Memoires de l'Acad. des sciences 1739, p. 468.

nerves, occasioning an uneasy sense of fulness, stoppage, or suffocation in the breast, which is more or less remarkable, according to the time during which respiration is stopped, the capacity of the pulmonary vessels, and the quantity of blood thrown into them by the right ventricle of the heart.

THAT a *stimulus* affecting the heart and alimentary canal should be the cause of their alternate contractions, as we have shewn above, is not improbable, the irritating cause being applied immediately to the organ to be moved; but that the diaphragm and intercostal muscles should be brought into contraction, by a *stimulus* acting upon the lungs, may at first appear unlikely, though, upon further consideration, we may assure ourselves of the reality of the fact, from the strongest analogy. Thus, for example; if a few drops of water, or any other liquor, by an accident in swallowing, fall into the *trachea*, the diaphragm and intercostal muscles are instantly brought into action, and continue to be agitated with alternate contractions and relaxations, till the stimulating cause is removed. Again, if a thin humour secreted in too great quantity, by the vessels and glands of the *bronchia*, distils upon the vesicles of the lungs, alternate convulsions of the diaphragm, intercostal and abdominal muscles ensue; which are repeated till the irritating cause is lessened or expelled. In a true peripneumony also, when, by reason of an obstruction in the pulmonary arteries, the blood passes through the lungs with great difficulty, a short cough is almost a constant symptom. Is it not therefore reasonable to infer, that a less *stimulus* or uneasy sensation in the vessels of the lungs, will be followed by gentler contractions of the inspiratory muscles?

AFTER expiration is finished, the blood beginning to be accumulated in the lungs, will, not only by its quantity stretching their vessels, but also by its heat, occasion an uneasy sensation, that is, act upon these parts as a *stimulus*; in consequence of which the diaphragm and intercostal muscles are contracted, and inspiration is performed; whereby the blood being not only cooled by the external air, but its passage being also promoted towards the left ventricle of the heart, the *stimulus* or uneasy sensation ceases: hence these

these muscles are relaxed; and consequently, by the re-action of the cartilages of the ribs, and the stretched abdominal muscles, &c. the cavity of the *thorax* is lessened, *i. e.* expiration is performed; which, on account of the disagreeable sensation which begins to be felt in the lungs, is soon succeeded by a new inspiration.

ALTHOUGH, in ordinary breathing, we are but little sensible of this uneasiness, arising from the difficult passage of the blood thro' the lungs after expiration is finished; yet if one attends to it, and restrains inspiration for some time, it becomes very perceptible: and as in asthmatic patients, the laborious contractions of the inspiratory muscles are, doubtless, owing to an anxiety and sense of suffocation in the breast; so it is reasonable to think, that in healthful people, the gentle *stimulus* of the warm blood accumulated in the pulmonary vessels, is the cause of ordinary inspiration.

FURTHER, a variety of appearances concur to persuade us, that the blood acting as a *stimulus* on the vessels of the lungs, after expiration, is the cause of the succeeding contraction of the diaphragm and intercostal muscles. Thus we observe, that as the blood flows in greater or less quantity through the lungs, inspiration and expiration more quickly or slowly succeed each other. Hence in a high fever, the breathing is much quicker than in health *, when one lies a-bed; and every one knows how both the pulse and respiration are accelerated by violent exercise. Though the quantity of blood flowing through the lungs remains the same, yet if its heat and bulk be increased, respiration becomes more frequent: hence in bagnios, and in the heat of summer, we breathe oftener than in our common rooms, and in more temperate seasons. Again, when any obstruction happens in the pulmonary vessels, which renders the passage of the blood thro' them more difficult than in health, respiration is more laborious and more frequently repeated: hence the

* In fevers, we sometimes meet with a very quick pulse, while yet the breathing seems to be scarcely quicker than in health; but in such cases, the pulse is small and quick, and consequently the quantity of the blood passing through the lungs may be little more than in health: when, although the motion of the heart be slower, yet it throws out more blood at every contraction.

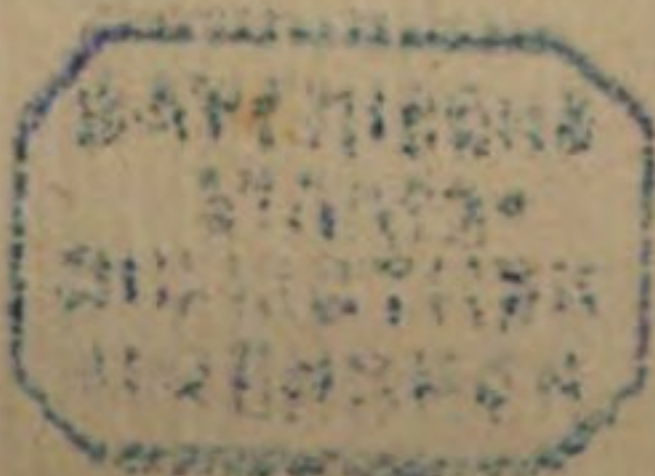
the quick breathing in peripneumonies, and other disorders consequent upon the obstruction of the lungs. If a portion of the lungs be rendered useless, or be wholly consumed by an ulcer, the patient is short-breathed and subject to asthmatic fits, upon the least fatigue, or upon any increase of motion or rarefaction in the blood.

SINCE therefore it appears, that the motions of respiration are, *ceteris paribus*, always proportional to the quantity of blood thrown into the pulmonary vessels, and its easy transit thro' them, this fluid ought to be esteemed the cause which excites, regulates, and continues these motions: and since respiration is more frequent and laborious, when a less quantity of blood passes with greater difficulty through the lungs, than when a larger stream flows thro' their vessels with more ease; these increased motions of the *thorax* cannot be owing to the inspiratory muscles being more plentifully supplied with blood and spirits, but must proceed from the *stimulus* or uneasy sensation accompanying the difficult passage of the blood thro' the pulmonary vessels, or its stagnation in them. And does not this plainly shew, why bleeding affords more speedy relief in fits of difficult breathing, than any other remedy?

2. If it be asked, how a *stimulus* or uneasy sensation in the lungs can affect the inspiratory muscles, with which they seem to have no immediate connection? I should answer, It were easy to ascribe this effect to a sympathy between their nerves; a phrase indeed oftener used than well understood! but as the pulmonic *plexus* has no greater connection or communication with the phrenic nerves, and those which supply the intercostal muscles, than with the nerves of the stomach, intestines, and other abdominal *viscera*, which are no ways affected by the gentle *stimulus* of the blood as it passes thro' the pulmonary vessels; I think we cannot fairly ascribe the motions of the inspiratory muscles to any sympathy proceeding from a connection or communication between their nerves and those of the lungs. Further, as the nerves of the inspiratory muscles and lungs most certainly do not terminate precisely in the same part of the brain, but probably in places somewhat distant from each o-

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ther, any sympathy that obtains between them, as proceeding from one common origin, must be owing to something equally present in these several places, *i. e.* to the mind or sentient principle: for without supposing some percipient BEING in the brain, how can an irritation of the extremities of the nerves, taking their rise from one part of that organ, occasion a more than ordinary derivation of spirits into such nerves as have their origin from a different part? If external objects act on the nerves only, by putting a stop to the equable progression of their fluids, or by exciting some vibratory motions in them, how can any of these occasion, not only a more copious derivation of spirits through the nerves thus affected, but also through a variety of other nerves with which they have no connection, and whose rise is from a different part of the brain? The sympathy, therefore, or consent observed between the nerves of various parts of the body, is not to be explained mechanically, but ought to be ascribed to the energy of that sentient BEING, which in a peculiar manner displays its powers in the brain, and, by means of the nerves, moves, actuates, and enlivens the whole machine.

BUT further, if the sympathy observable between different parts of the body, be wholly owing to the connection or communication of their nerves, how comes the pupil to be contracted by the action of light on the *retina*, when the nerves of the *uvea* have not only no communication with the optic nerve, but arise from a pretty distant part of the brain? or, if there were some general sympathy between the nerves, why should not the longitudinal fibres of the *uvea* be contracted, as well as the orbicular ones, and so the coarctation of the pupil be prevented? If the alternate contractions of the inspiratory muscles were owing merely to their receiving a few nervous twigs from the intercostals, which furnish the *plexus pulmonicus*, why is not the heart and alimentary canal equally affected with them, by a *stimulus* or uneasy sensation in the lungs? why are not the intercostal muscles as much convulsed in vomiting as the diaphragm and abdominal muscles? and why, upon an irritation of the membrane of the nose and *trachea*, are not the abdominal muscles contracted, till the inspiratory muscles begin to be relaxed? These questions

questions will scarce be answered satisfactorily, upon any scheme of sympathy depending wholly on the communication or connection of nerves; but have no difficulty in them, if the motions now mentioned be referred to the mind or sentient principle.

WHEN, therefore, in consequence of a disagreeable sensation in the lungs, arising from the difficult passage of the blood through their vessels soon after expiration is finished, the inspiratory muscles are contracted; we are not to ascribe this to any unknown sympathy acting mechanically upon these muscles or their nerves; but to the MIND or sentient principle, which being affected by the uneasy perception in the lungs, is thereby excited to increase the action of the nervous influence upon the intercostal muscles and diaphragm; by which the cavity of the *thorax* being enlarged, and the lungs inflated with fresh air, the disagreeable sensation in them is removed; and consequently the extraordinary contraction of the inspiratory muscles ceases: hence, by the re-action of the elastic cartilages of the ribs, abdominal muscles, &c. the cavity of the *thorax* is lessened, *i. e.* inspiration is naturally followed by expiration; which again must soon be succeeded by a new inspiration, on account of the particular sensation which begins to arise in the lungs.

3. IT does not appear, that any effort of the mind or sentient principle is necessary to expiration; for this naturally takes place as soon as the muscles of inspiration cease to act. The re-action of the cartilages of the ribs and stretched *pericardium* and *peritonæum*, are wholly owing to the elasticity of the parts, and not to any muscular contraction or additional force communicated to them at this time; nay, the re-action of the abdominal muscles in expiration, seems to be pretty much of the same kind; altho' it is not improbable, that the extension of their fibres, by the depression of the diaphragm in inspiration, may, as a very gentle *stimulus*, excite in them a true muscular contraction*. However, this irritation prompting the

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* The same thing may be said of the sternocostal muscles, which are generally thought to pull the *sternum* and cartilages of the ribs downwards and backwards in expiration, but which seem scarcely, if at all to act, in ordinary and gentle expiration.

abdominal muscles to contract at the end of inspiration, must be inconsiderable, and in no ways like the convulsive motions brought on other muscles by a *stimulus*; since, when they are greatly stretched by a very full inspiration, we do not find any difficulty in preventing their contraction, or in moderating it as we please; while, on the contrary, in vomiting, their motions are altogether convulsive and involuntary. Further, the *thorax* of dead animals being in a full state of expiration, shews that this is brought about, after all muscular action ceases, by the mere elastic force of the parts. Nor is it any objection here, that when air is blown into the *thorax* of a dead body, expiration is performed more slowly than in a living one; because, by the cold and total stagnation of the fluids, the parts not only become more rigid, and the articulations of the ribs less moveable, but the instruments of expiration lose, in a good measure, their elastic power.

LASTLY, The appearances observable in comatous patients, and which we shall hereafter take notice of, shew, that whereas inspiration is owing to the energy of the sentient principle, and is, therefore, in such cases, in some degree interrupted, expiration is performed as usual; which could not happen, if, like inspiration, it proceeded from the mind, or from any particular sensation directing it to put certain muscles in action; for in that case there ought to be a pause at the end of inspiration, as well as at the end of expiration. Upon the whole, therefore, we may conclude, that expiration naturally ensues as soon as ever the inspiratory muscles cease to act, chiefly by virtue of the elasticity of the stretched parts, and scarcely by the power of any muscular contraction *.

HERE we may observe a remarkable analogy betwixt the motions of the pupil, and those of the *thorax* in respiration. The contraction of the pupil, when light is admitted into the eye, is owing to the muscular contraction of the circular fibres of the *uvea*, in which, therefore, it seems to correspond with inspiration; but its relaxation,

* What is said here, is only meant of ordinary and gentle expiration, which is performed without consciousness, or the assistance of any of those muscles which act in violent expiration.

relaxation, when the *stimulus* of light ceases, is produced merely by the natural contraction of the longitudinal fibres of this membrane; and consequently in this it resembles expiration.

4. If it be demanded, why, after the lungs, by an effort of the will, have been kept for some time in a state of full inspiration, the inspiratory muscles are immediately relaxed, when this effort ceases, and not rather continued in a state of contraction, seeing there is an uneasy sensation in the lungs, equal to what happens after expiration? the answer is, That the disagreeable sensation is of a different kind, and which the keeping the lungs in a state of inspiration would tend rather to increase than remove: for such is the constitution of our frame, and the law of that wonderful union between the soul and the body, that the former, upon any uneasy perception, produces such motions and changes in the latter, as naturally and most effectually tend to lessen it, or expel the irritating cause. Thus the uneasy sensation arising from the action of light upon the tender *retina*, is not followed by a contraction of the longitudinal fibres of the *uvea*, because this would not have the effect of diminishing, but of adding to the disagreeable perception. For the same reason, the mind, in consequence of a *stimulus* or uneasy feeling in the lungs, increases the action of the nervous influence upon the intercostal muscles and diaphragm, and not upon the muscles of the *abdomen*, back, or loins. But further, the continuing the inspiratory muscles for any considerable time in a state of full contraction by a voluntary exertion of the power of the mind, never fails to give some degree of uneasiness; so that it is no wonder, if they be relaxed as soon as the effort of the will ceases.

5. SUDDEN surprise, or any thing that will but for a few moments strongly fix the attention of the mind, prevents the hiccup, if it be from a weaker cause. Sneezing is stifled in the beginning, whenever an attempt to perform it raises an acute pain in any part of the body; as frequently happens in rheumatisms affecting the muscles of the back or *thorax*. In these cases, the mind, being strongly affected

fectcd by the acute pain, becomes, in a great measure, insensible of the irritation of the olfactory nerves *; wherefore no violent or convulsive motion ensues. In the same manner, if there be only a gentle irritation in the *trachea*, any thing that renders the mind less sensible of this, will prevent coughing. Hence, in time of eating, a tickling cough generally ceases, or is less frequent; for not only the chewing and swallowing of the food engage the attention of the mind at that time, but the motions of the *larynx* in deglutition have almost the same effect in lessening the sense of irritation in the *trachea*, as friction has in diminishing a small degree of pain, or itching, in any part of the body. As therefore the convulsive motions of coughing, sneezing, and the hiccup, are undoubtedly owing to an uneasy sensation affecting the mind, may we not conclude, that the gentler *stimulus* of the blood, in the vessels of the lungs, becomes, through the intervention of the mind or sentient principle, the cause of the less violent motions of the inspiratory muscles?

6. THIS will still further appear, from considering respiration in some morbid cases, and in animals placed in a receiver partly exhausted of air.

* IN acute diseases, where the head is much affected, a remarkable alteration often happens in the breathing: expiration indeed succeeds inspiration in the usual way; but sometimes, after expiration is finished, there is a long pause before a new inspiration is begun. In a patient, whose brain was affected from an *ischuria*, I observed this interval between the end of one expiration, and the beginning of a subsequent inspiration, to be many times from seven to ten seconds: but in a young woman, who died apoplectic, inspiration sometimes did not succeed the expiration, till after I had counted 20, 30, 40, or even more strokes of my pulse, which did not beat above 75 times in a minute. This circumstance, which is easily

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* How a stronger sensation should destroy, or render the mind insensible of a weaker one, is a question attended with some difficulty; concerning which see below Sect. xi. answer to objection 2.

ly explained, upon the principles which we have laid down, is altogether inconsistent with every mechanical account of respiration that has hitherto appeared.

IN these cases, the brain, and common *sensorium*, being greatly affected, the mind or sentient principle must have been much less sensible, than it usually is, of any impression, irritation, or *stimulus* affecting the nerves. Hence, after expiration, which, from the relaxation of the cartilages of the ribs, &c. naturally, and without the intervention of the mind, succeeds inspiration, a long pause intervenes before a new inspiration comes on; because the mind is not roused to exert her influence, till the uneasiness and sense of suffocation in the breast becomes so considerable, as to awake her, as it were, out of a profound sleep. Let any one, after expiration is finished, keep his *thorax* in that state for half a minute or more, and he will feel, before that time is elapsed, such an uneasiness in his breast, as will beget in him the strongest desire of dilating the lungs, and taking in fresh air.

IT is plain, that, after such a stop, if Boerhaave's theory were true, inspiration never could succeed, because it must necessarily occasion a greater obstruction to the motion of the blood through the lungs, than can ever happen at the end of inspiration, and consequently render the inspiratory muscles unable to overcome their antagonists, and dilate the *thorax*. If respiration were owing to the alternate compression of the phrenic nerves, would not these nerves, on account of the blood accumulated in the vessels of the lungs, be more compressed 20 or 30 seconds after expiration, than when it was just finished, and consequently be rendered then more incapable to actuate the inspiratory muscles?

THE truth of these principles will be further confirmed by the following case. A girl of five years of age, having, at 7 o'clock in the evening, swallowed, by mistake, about a dram and a half of liquid *laudanum*, soon became merry, and laughed, then delirious, and in half an hour was seized with a drowsiness and *stupor*; at 10 her breathing was high, with a snoring noise, her pulse full and equal, though slow; she could not be fully awaked, but looked up a little, and

and seemed to be sensible of pain, when severely pinched: about 11 her face grew pale, her eyes were fixed and glazed, and her breathing would often gradually decrease, and at last stop, for near a minute; then it began again with a deep inspiration and sighing. At first when the breathing was thus interrupted, the intervals were shorter, but grew gradually longer till the patient died. While the motions of respiration were decreasing, the pulse became smaller; and when they stopped, it was very weak and slow; but equable and without intermissions: when respiration began to be renewed, the pulse recovered its strength, and became quicker.

THE interrupted breathing is accounted for, from the *stupor* and insensibility which *opium* occasions when taken in too great quantity; and the intervals becoming gradually longer, was owing to the senses being more and more lock'd up by the further action of the *laudanum*, till at last, the mind becoming insensible of the *stimulus* or sense of suffocation in the lungs, a final stop ensued. Add to this, that from the great weakness and slowness of the pulse while respiration was suspended, its return could not well be owing to any mere mechanical cause; for the secretion of spirits, and every function of the body depending on the general circulation of the fluids, must have been more languid immediately before the renewal of respiration, than when it began to cease. We shall not be able to account for the appearance now mentioned from any compression of nerves, or alternate oscillations of a highly elastic fluid in the fibres of the inspiratory muscles; for when the heart itself was sinking, and all motion in the body was ready to cease, every mechanical power sufficient to excite respiration must have been less able to renew this motion, than it was a little before to have continued it. Lastly, since the pulse was at all times slow, and in the intervals of respiration not only weaker than usual, but also more remarkably slow, it appears that the heart was in some measure rendered less obedient to the *stimulus* usually affecting it *. Nor is this surprising; since, from the dissections of living animals, we know that a large dose of *opium* almost entirely suspends the peristaltic motion of the stomach

* See below, Sect. xiv. No. 24 and 25.

mach and intestines *. The heart seems to be endued with a much greater degree of sensibility than the lungs; and this perhaps may be the reason, why its motion was less disturbed by the *stupor* occasioned by the *laudanum*, than that of respiration; which, besides, is performed by muscles whose fibres or membranes have no *stimulus* immediately applied to them.

* IN the *Edinburgh Medical Essays*, vol. 5. art. 55. we are informed, that, after blowing into the lungs of a man, who had been dead to all appearance for above half an hour, the *thorax*, which by this means was elevated a little, continued alternately to rise and fall, gradually acquiring greater degrees of motion, till at last respiration came to be as fully performed as in healthful people.

COULD this be owing to any mere mechanical powers in the body? I think not. A machine adjusted according to the most exquisite rules of art, though it might for some time have preserved the motion communicated to it, could never of itself have generated a greater motion! In order therefore to account for the renewal of respiration in this case, we must have recourse to the energy of the SENTIENT PRINCIPLE, exerted, here, in consequence of the motion imparted to the fluids in the lungs, by their first inflation.

† THOUGHTFUL melancholy people, whose minds are fixed upon some particular object, being thereby less affected by the *stimulus* or slight uneasiness which begins to be felt in the lungs, after expiration is ended, usually breathe more slowly, and after longer intervals, than those who are in perfect health; by which means, the blood passing less freely through the pulmonary vessels, and being accumulated in them, a sense of weight and suffocation arises, which more powerfully affects the mind, obliging them, often, to draw in a more than ordinary quantity of air, and occasioning what is usually called a deep sigh.

By what mechanism can it be, that in the half-exhausted receiver, animals breathe quicker and higher than in the open air? Upon Boerhaave's principles, the motions of respiration ought not

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* Kaau Impet. faciens, No. 434. and 435.

to be redoubled in this case, since the blood flows with greater difficulty through the lungs. And if, according to Swammerdam and Pitcairn, the alternate motions of the *thorax* were owing to the inspiratory muscles having no antagonists, how could this be altered by the air's being rendered lighter; or how could this or the stagnation of the blood in the lungs make these muscles repeat their contractions more strongly and frequently? But it is evident, upon the theory which we have offered, that in proportion as the air in the receiver is exhausted, and respiration becomes more difficult, the mind must increase its efforts, in order to dilate the lungs more fully, and to get rid, if possible, of that anxiety, or sense of suffocation, which always accompanies the stagnation of the blood in the pulmonary vessels, or its difficult passage through them.

3. LASTLY, Upon what *hypothesis*, founded wholly on the received properties of bodies and the laws of motion, can it be shewn, that the frequency and fulness, slowness and smallness of respiration should, in healthy people, be constantly in proportion to the heat and cold, rarity and density of the air?

7. RESPIRATION differs from most of the other spontaneous motions, in being subject to the power of the will: thus we can at pleasure accelerate, retard, or put a stop, for some time, to the motions of the intercostal muscles and diaphragm: nor is this power of the will over these muscles owing (as Boerhaave supposed *) to the mind's preventing their alternate contraction, by means of the stronger voluntary muscles, which are employed in laborious respiration; for one, by making the experiment, may soon satisfy himself, that, without the assistance of any other muscle, he can, when he pleases, either contract the diaphragm with different degrees of force, or stop its motion altogether. But though respiration thus differs from the proper involuntary motions, yet it does not perfectly agree with those that are voluntary, since it is regularly performed in time of sleep, and when we are awake, although we be not conscious of it.

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* Institut. med. No. 624.

THE motions of the intercostal muscles and diaphragm are not, like those of the heart and intestines, independent on the will, because the *stimulus* exciting their action is applied to a distant part, and not to the organs moved; and while such a *stimulus* is gentle, and the part affected by it not very sensible, as is plainly the case of the lungs, the contraction of the muscles that is wont to follow it may be prevented by the interposition of the will; but if the *stimulus* and uneasy sensation be much increased, the usual motions ensue, in spite of any determination of the will to the contrary. Thus, when the membrane of the *trachea* is only slightly irritated, we can restrain coughing; but when it is more strongly affected, all endeavours to hinder it are in vain. When an ordinary *stimulus* moves us to stool, or to make urine, tho' the diaphragm and abdominal muscles be, as it were, spontaneously contracted, yet we can restrain their motions if we please; but in a violent *tenesmus* or strangury, they are convulsively contracted, notwithstanding any effort of the will to the contrary. In like manner, the *stimulus* exciting the usual contraction of the inspiratory muscles, after expiration is finished, is so gentle, that we can at pleasure prevent its taking place; but in severe asthmatic fits, where the uneasy sensation is much increased, the will begins to lose its power of restraining the motion of these muscles; nay, even some of the voluntary muscles, at this time, are forced into action for their assistance: and if, in such cases, the will can at all stop the motions of the inspiratory muscles, it is not so much by its immediate power over them, as by means of the stronger muscles employed in voluntary respiration.

It must indeed be acknowledged, that altho' the contraction of the orbicular muscle of the *uvea* be not owing to any *stimulus* immediately applied to it, but to the irritation of a distant part, yet it is altogether independent on the will; and in this differs from respiration, and from the motion of the muscles employed in coughing, and in voiding the urine and *feces*. Perhaps the reason of this difference may be, that the action of light upon the very sensible *retina* affects the mind so strongly, that we cannot, by any power of the will, prevent the contraction of the pupil: Further, it

is not impossible that the will, merely thro' disuse, may have lost its power of restraining the motions of the muscular fibres of the *uvea*, even when the *stimulus* is ever so gentle *.

BUT whatever may be the efficient cause which thus subjects respiration to the government of the will, the final cause of this difference between it and the other vital motions is pretty evident: for were it not that the motions of the muscles employed in respiration may be varied at pleasure, we should not only be unable to evacuate the urine and *feces*, but must have been deprived of speech.

^b IF it shall be objected, that the motion of respiration cannot be owing to the mind or sentient principle; because it obtains at all times, and is kept up when we are asleep and not conscious of it, equally as when we are awake and attentive; I would observe that a variety of actions are performed by the influence of the mind, without our adverting to them. For instance: The eye-lids never cease, after short intervals, to move, whether we be sensible of this or not; nay frequently, as when any thing threatens the eye or touches the *cornea*, they move whether we will or no; and yet these motions are owing to the mind. Why, therefore, may not respiration be carried on much in the same manner, without our attending to it; especially since we have shewn that the difficult passage of the blood thro' the lungs, and the uneasy sensation thence arising, must at all times influence the mind to continue this action?

IN time of sleep, do we not often swallow the *saliva*, talk, move our limbs, and change the posture of our bodies? Nay some get out of bed, and walk from one room to another. Here, then, are actions performed in sleep, which, nevertheless, must be ascribed to the action of the mind. Further, in cases where breathing is difficult, when the patients are not conscious of it, nay even in sleep, respiration seems to be performed, partly by the assistance of other muscles besides the proper inspiratory ones, *i. e.* by muscles which commonly are not employed except in voluntary motion, and whose action, therefore, in such extraordinary cases, must be attributed to the mind alone. How, then, can it be urged, that the motions of the

* See above Sect. 1. No. 18.

the diaphragm and intercostal muscles, since they continue to be performed while we are asleep, are not owing to the mind? If, while a child is asleep, and breathing softly, a thick cloth be laid over its face, it will begin to breathe deeper or quicker, and so continue to do till, the cloth being removed, the cool air is admitted into the lungs. This must arise from the uneasiness which the mind feels from the difficult passage of the blood thro' the lungs, and the want of proper air; and, in order to get rid of these inconveniencies, it moves the *thorax* with uncommon force and frequency, whereby a greater quantity of air is inspired, and the lungs are more dilated. In an *asthma*, does not the sense of suffocation excite the mind to redouble the motions of the inspiratory muscles, and are we not sensible of this when awake? In sleep, when we are less conscious of this uneasiness, does not the mind continue to exert its influence in the same manner upon these muscles? And this I should take to be no small proof of the mind, as a sentient principle, being often affected by what passes in the body, and in consequence thereof, excited into action, when, in the mean time, we do not advert to any such thing.

S E C T. IX.

Of the beginning of respiration in animals.

AS it would be unnecessary to prove, that the *fetus* cannot perform the action of respiration, when it is in the womb, and inclosed in its membranes; I shall take it for granted that animals do not begin to breathe, till the external air has access to them, at the time of birth; tho' I find, not indeed without surprise, some of the moderns going into the contrary opinion*.

HOWEVER presumptuous it may appear to attempt the solution of a problem, in which some of the greatest physiologists have failed, I must beg leave, however, to say, that I think it as easy to account
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* Mazini opera, tom. 3.

for the beginning of respiration, as for its continuance when once begun; and that both are owing to the same cause, namely, to an uneasy sensation in the lungs.

OUR bodies, framed, as they are, with the most exquisite skill, would soon perish if deprived of aliment and air. The former must be supplied at short intervals, but the latter, almost every moment. The *fœtus*, while in the womb, stands in no need either of the one or the other; the mother's juices, transmitted to it thro' the vessels of the *placenta*, supplying the want of aliment; and the peculiar circumstances of its heart making the alternate motion of respiration, which is requisite for carrying on the circulation of the blood in animals after birth, unnecessary here; while the mother's fluids, having undergone the action of the air in her lungs, are duly fitted for all the purposes of the *fœtus*, without any respiration of its own. The necessity, therefore, of air and aliment commences with our birth; and as we are excited to take in meat and drink by the uneasy sensations of hunger and thirst, which, as faithful monitors, never fail to warn us when these are wanted, but immediately cease upon the appetite's being satisfied; so, to prevent our being in danger of perishing thro' the want of fresh air, there arises, unless the action of breathing be continually repeated, and new supplies of fresh air thus brought into the lungs, an uneasy sensation, which may not improperly be termed the APPETITE of breathing.

IF then an appetite for fresh air be as natural to animals after birth, as a desire for aliment; and if none ever thought of accounting either for the sensations of hunger and thirst, or for the taking of food consequent upon them, merely from the mechanical construction of the stomach, gullet, and *fauces*, without having recourse to the mind; why should we attempt to explain the action of respiration from principles purely mechanical, and deny the perception and operation of a sentient active principle to be the cause which at first begins, and ever after continues it? I should think that the analogy were too strong not to strike every unprejudiced mind!

BUT

BUT here it may be asked, in the way of objection, if the *fœtus*, during its confinement in the womb, has an inclination to breathe, why is not the *liquor amnii*, like the air after birth, received into its lungs, and expelled alternately *: The answer to which is,

I. THAT, had the *fœtus in utero* ever so strong a desire to perform the action of respiration, it could not admit the *liquor amnii* into its lungs; partly because water is a fluid improper for the purposes of respiration, but more especially because, whenever it touches the top of the *larynx*, the *glottis* is so constricted, by the convulsive contraction of its muscles, that scarcely can one drop pass into the lungs. This appears from observing that those animals, which have been immersed and kept under water, do not admit any of that fluid into their lungs, much less such a quantity of it as might be esteemed the cause of their death †. If, therefore, a little water has at any time been found in the lungs of drowned animals, we are not to imagine that it made its way thither while they were alive, but only after their death, when the muscles of the *glottis* losing their power of contraction and becoming flaccid, the sides of this passage recede from each other.

THE different cravings or appetites have their several corresponding objects; fresh air, on account of its cooling quality, and because it equably and gently dilates the lungs, and thereby promotes the circulation of the blood thro' their vessels, is as well adapted to satisfy the appetite of breathing, as thin cooling drink is to quench thirst; whereas water would be altogether as improper for the purposes of respiration, as dry bread for diluting our fluids, or satisfying the cravings of a thirsty animal. Hence, as food has the highest relish to a hungry person, so, to one half-suffocated for want of air, nothing is more refreshing or more greedily taken in than that fluid.

SINCE,

* Dr Martine has proposed it as a problem; why the *fœtus* in time of gestation never dilates its *thorax*, nor at any time before birth performs, however slowly, the motions of inspiration and expiration successively? Medical Essays, vol. 1. art. xii.

† Histoire de l'Acad des sciences 1719, edit. 8vo, p. 32. and 1725, p. 16.

SINCE, therefore, it appears from what has been said, that water is unfit for satisfying the appetite of breathing, and that animals which have been long accustomed to respiration never attempt, when under water, to draw it into their lungs; it follows, that the *fœtus in utero*, even supposing (what seems not, however, to be the case) it had the same appetite of breathing with animals after birth, could not receive the liquor of the *amnios* into its lungs, and expel it thence alternately. Thus we see how easy a solution Dr Martine's problem admits of; and that, for this purpose, it is unnecessary to have recourse to any compression of the phrenic nerves.

2. BUT it seems probable, that either the appetite of breathing does not take place in *fœtuses* till birth, or that it becomes then much stronger.

“ FOR the uneasy situation of the *fœtus*, when the birth approaches, and its various motions and struggles in consequence of its uneasy situation, must quicken the circulation of the blood thro' its vessels; whereby the lungs will not only receive a larger share of fluids in a given time, than formerly, but the blood passing in greater quantity than usual from the left ventricle of the heart into the *aorta*, and making thereby a stronger resistance to the passage of this fluid thro' the *ductus arteriosus*, it will then be more copiously determined into the pulmonary artery, from whose beginning this duct arises. But if the blood be thus pushed in greater streams into the vessels of the lungs at the time of birth, and even somewhat earlier, will there not hence ensue a greater degree of heat, and fulness, a more active *stimulus* and uneasiness in these vessels; or, in other words, will not the appetite of breathing be hereby increased*? After birth, when the *fœtus* remains for some time inclosed in its membranes before it is allowed to breathe, this increased motion of its blood may well be supposed to be abated; however

* Altho' immediately before birth the *fœtus* may be so pressed upon and confined by the parts surrounding it, as to prevent its making any considerable motions, yet the pain as well as the strong alternate pressure which it suffers, cannot fail, at that time, to quicken the circulation of its blood.

however the cold, which it is now more exposed to than formerly, by contracting the placentary and cutaneous vessels, will accelerate the return of their blood to the right ventricle of the heart, and thereby increase the force of the circulation in the interior parts of the body.

CHILDREN which are cut out of their mother's womb, often remain a considerable time without attempting to breathe, because there is no such increase of the motion of the blood thro' their lungs, as in children which are brought forth in the natural way, and which therefore must have a greater inclination to inspire. It is to be observed, however, that, in some cases, weakness, and consequently a languid circulation thro' the lungs, is the cause why children delivered in the usual manner, as well as those who are cut out of the *uterus*, do not begin to breathe for some time after they are exposed to the air.

It has been observed, that when the *uterus* of a living bitch is opened, the puppies inclosed in their membranes remain quiet for some time; but, about the time of the mother's death, they begin to move and struggle, as if affected by some uneasy sensation; and that if their membranes be laid open at this time, or a little after, so as that the air be admitted to them, they immediately begin to breathe, and seem to be relieved from what before oppressed them; but that, if this be not done, they quickly begin to languish, and at last die*. Does not this argue that a necessity of respiration, and a desire, stronger at least than before, of dilating the *thorax* for the admission of air, commences on the part of the *fœtus*, immediately after its intercourse with the mother is cut off by the birth?

As the water surrounding the *fœtus* would hinder its efforts to inspire, how much sœever it were desirous of so doing; so the air, the proper *medium* for respiration, being applied after birth, to the face, mouth, and nostrils, may excite new efforts to breathe, an ac-

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* "Fœtum cum suis involucris eximo, et exteriori effracto involucri per interioris valde pel-
lucidum ostendo qui fœtus respirare conetur, quamque pulchre interiori quoque fracto arëem
ducatur." Vesalii Anatom. lib. 7. c. 19.

tion which, by the peculiar sensation in its lungs, it must be much disposed to. We may observe that a new-born child rarely attempts to suck except something be applied to its mouth; and, perhaps, in the same manner the appetite of breathing itself may be increased in a *fœtus* by the presence of a fluid so well answering its demands. The chick, after it becomes large and strong, is observed to gape when the shell is opened; and does not this indicate a desire of breathing? For this opening of the mouth is not for swallowing the liquor of the *amnios*, since the chick is nourished, not by the mouth, but by the umbilical vessels alone *.

UPON the whole, altho' it is not improbable that the *fœtus*, during its stay in the womb, may, especially towards the end of gestation, from the difficult passage of the blood thro' the compressed lungs, have a tendency to breathe, yet surely this appetite of breathing must be greatly increased at birth. Nor is breathing the only action performed by a new-born child, which it was a stranger to before, since sucking is equally new to it as breathing †. The former has been generally referred to instinct, and so perhaps may the latter: but as I would decline the use of words whose meaning may be obscure or indeterminate, I should say, that sucking and breathing are owing to particular sensations in the body, determining the mind or sentient principle to put certain muscles or organs in motion.

HAVING thus accounted for the beginning of respiration in animals, we shall, in a few words, mention what has been advanced on this head by some of the most eminent writers in physiology.

PITCAIRN derives the first inspiration in new-born animals from the air rushing, by the *glottis*, into the cavity of the *thorax* at birth, as it were into a *vacuum*, and thus enabling the intercostal muscles to raise the ribs ‡. But as the lungs, before birth, fill the *thorax*, there can be no *vacuum* into which the air can rush: for, if there were,

* See Edinburgh Medical Essays, vol. 2. art 10.

† That the *fœtus in utero* does not swallow the *liquor amnii*, has been so clearly demonstrated by my learned Colleague Dr Alexander Monro senior, that I think it one of the few points in Physiology which ought for the future to pass undisputed. See Edinburgh Medical Essays, vol. 2. art. 9.

‡ Differt. de caus. qua sanguis fluit per pulmon. sect. 14. p. 53.

were, the *liquor amnii* must have been forced into it, while the *fœtus* was in the womb; and in dead-born *fœtuses*, the air ought to rush into the lungs and expand them; both which circumstances are contradicted by experience. He adds, *Irrumpit, inquam, aer velateris et gravitatis, non autem dilatati prius pectoris, compulsus* *; whence it appears, that the celebrated author was not sufficiently acquainted with the true manner in which inspiration is performed: for it is owing, as has been shewn above, to the enlargement of the cavity of the breast, made by the contraction of the intercostal muscles and diaphragm. Besides, since the *thorax* is in a state of full expiration in all dead animals, it appears that the air cannot, by its gravity, &c. dilate the lungs; but that, in order to the first inspiration, the diaphragm and intercostal muscles, by their contraction, must enlarge the cavity of the chest.

THE great Boerhaave, after Thruston and Borelli, ascribes the beginning of respiration to the *fœtus* moving all its muscles violently in the time of birth, and, among the rest, the intercostals and diaphragm †. But this account is not satisfactory; since Vesalius and Boyle have observed, that puppies cut out of their mother's womb begin to gape and breathe as soon as they are exposed to the open air ‡. And when infants, which seemed to be dead-born, have begun soon after to breathe, we are not, with an illustrious author, to ascribe this to the action of any secret spring, or undiscovered piece of mechanism, bringing the muscles of inspiration at this time into play, but to the energy of the sentient principle, which, as soon as the heart begins to vibrate, is roused by a disagreeable feeling in the lungs, to dilate the *thorax* and take in air. The cause, therefore, of the first inspiration in this case, appears to be the same as that of its recommencement after a *syncope*, namely, the blood pushed by the heart, upon its recovering motion, into the pulmonary vessels, and there acting as a *stimulus*.

P 2.

ANOTHER

* Differt. de caus. qua sang. fluit per pulmon. sect. 15. p. 54.

† Institut. Med. § 691.

‡ Vesal. Anatom. lib. vii. cap. 19. and Boyle's Exp. phys. mechan. p. 41.

ANOTHER learned author deduces the beginning of respiration from the endeavours of the *fœtus* to cry, upon account of its uneasy situation and the pain it suffers in the time of birth. But, if the commencement of breathing were owing to no other cause than this, why should not this action cease soon after the child is delivered, when it is free from pain and gives over crying? Or why should it begin in Vesalius and Boyle's experiments above mentioned, where the usual causes exciting the animal to cry were wanting? And why should the *fœtus* shew such marks of anxiety when inclosed in its membranes, and be quickly at ease, upon its having access to the air and being allowed to breathe?

BUT what is crying in infants? It is no more than an irregular kind of breathing, which affects the expiration chiefly, and is owing to some painful sensation; for, hence the air being forcibly expelled through the *glottis*, which at this time is constricted more than usually, produces that noise called crying. Saying, therefore, that the beginning of respiration in animals is owing to their attempting to cry, is, I should think, little more than, in other words, to say, that it depends upon a painful sensation, which, in animals accustomed to breathe, prevents the muscles of respiration from being moved in a regular and natural way. But, as the action of these muscles is performed in a more regular and equable manner as soon as the cause of crying ceases, it seems more reasonable to believe that the first inspiration is owing to the same cause as the second, third, and every succeeding one, namely, to a particular sensation in the lungs affecting all new-born animals; while the pain, which occasions crying, is merely accidental, and seems not to give rise to respiration, when it does happen; though, after it is once begun, it is the cause of its being performed after an irregular and interrupted manner.

ANIMALS, when drowned, or suffocated with bad air, are often brought to life again by friction, agitation, or by blowing air into their intestines or lungs; all which expedients, as they communicate motion to the blood stagnating in the great veins adjoining to the heart, tend to renew the contractions of that organ,
and

and consequently the circulation of the blood through the vessels of the lungs, to which motion alone the recommencement of breathing is owing, and not to any attempt to cry or howl, which, in these cases, is seldom observed.

FURTHER, in bats, hedge-hogs, and other animals which lie in a death-like state during the cold of winter, and without any alternate motion of their *thorax*, can the recommencement of breathing, in the spring, be ascribed to any painful sensation exciting them to cry? I should think not; but to the returning heat of the sun agitating their fluids, and communicating a new motion to their heart, whereby the blood is pushed, as is usual in living animals, into the pulmonary vessels, and where, chiefly on account of its difficult passage, it excites a peculiar sensation, which rouses the soul or sentient principle, as it were, from its state of indolence and inactivity, to contract the inspiratory muscles, and thereby perform the action of respiration.

UPON the whole, therefore, I presume to think that the beginning of respiration in new-born animals, or the recommencement of it in those in which it has been for a long time interrupted, cannot be deduced from an inclination to cry *; but is owing to a peculiar sensation

* The learned author, in another part of his works, (published since the first edition of this essay), not only ascribes the beginning of respiration in animals to their endeavouring to cry at the time of their birth, but chiefly to their having been accustomed, while in the *uterus*, to swallow the liquor of the *amnios*; whence they are no sooner exposed to the air, than they draw in this fluid, with which they are now surrounded.

But supposing, what is not probable, that the *fœtus*, while in the womb, had been in use to swallow the liquor contained in the *amnios*, it will not follow that it should inspire, or draw the air into its lungs, as soon as it is born; for the action of inspiration is very different from that of deglutition, and is performed by different instruments and organs. If the *fœtus* had been accustomed to swallow before birth, it might indeed, immediately after, be apt to move the organs of deglutition, and swallow its own *saliva*; but how should this action occasion inspiration? The motion of the *larynx* and *fauces* in deglutition, can have no influence in producing a contraction of the diaphragm or intercostal muscles: And we do not find that hunger, or a strong desire to swallow either liquid or solid food, ever increases that inclination which animals have to perform the action of inspiration; on the contrary, after we have fasted long, we breathe seldomer than after a full meal. In like manner, when, in consequence of being half suffocated by breathing in too confined an air, we have a greater desire than

sensation in the lungs, which, as it at first gave rise to this action, so it is the cause of its being ever after continued. And if we are so formed, that we feel a craving appetite, as often as our bodies require a new supply of food, and a different sensation when our fluids want dilution, can it be thought strange that an appetite should be given us for air, when the deprivation of it would become much sooner fatal?

A Solution of HARVEY'S Problem.

“QUI fit, ut foetus in lucem editus, ac membranis integris
 “**Q**uopertus, et etiamnum in aqua sua manens, per aliquot
 “horas, citra suffocationis periculum, superstes fit: idem tamen
 “*secundis* exutus, si semel aërem intra pulmones attraxerit, postea
 “ne momentum quidem temporis absque eo durare possit, sed con-
 “festim moriatur *?”

THIS problem, which was first proposed by the great Harvey, appears now to admit of an easy solution.

THE *fœtus* lives in the womb without respiration, because the greatest part of the blood, by means of the *foramen ovale* and *ductus arteriosus*, is conveyed from the right *sinus venosus* and ventricle of the heart into the left ventricle and *aorta*, without passing through the

than usual to inspire, we do not find that our sense of hunger, or inclination to swallow, is thereby increased. And indeed, it would seem that a desire to swallow is as different from, and has as little connection with an inclination to breathe, as a sense of hunger in the stomach has with a sense of suffocation in the lungs.

In animals, which have been drowned, or suffocated by bad air, respiration is restored by friction of the *abdomen*, blowing into the *anus*, or by any other means that can renew the motion of the blood through the heart and lungs: In this case, therefore, the recommencement of breathing cannot be owing to a sense of hunger, or a desire to swallow.

Further, if, as I have endeavoured to prove, and as the learned author himself allows, the continuance of respiration in animals be owing to an anxiety or disagreeable sensation in the lungs arising from the slow passage of the blood through their vessels; is it not more reasonable to ascribe the beginning of respiration in new-born animals to such an uneasy feeling, than to an inclination to swallow?

* Harvey De generat. animal. cap. de partu, p. 501.

the lungs; and because its fluids, being derived from the blood of the mother, which has sustained the action of the air in her lungs, must be equally fit for its nourishment and support as for her's. When the *fœtus*, after being separated from its mother, remains in its secundines, it lives for a considerable time without breathing; because the circulation of the blood continuing in the same manner as when it was in the womb, only a small portion of it passes through the lungs.

AFTER birth, when the *fœtus* has once been accustomed to breathe, it soon dies, if respiration be discontinued *; because the blood, which formerly went by the *foramen ovale* and *ductus arteriosus*, passes now through the vessels of the inflated lungs; and although we cannot suppose these passages to be instantly shut after breathing begins, yet as the vesicles of the lungs, after having been once inflated, never collapse so far as to occupy as little space as before, their vessels must go on to receive a greater quantity of blood than before birth, and consequently to transmit this fluid in greater abundance to the left *sinus venosus*; by which means the passage of the blood into this *sinus*, by the *foramen ovale*, will, in a great measure, be prevented; at the same time, by the inflation of the lungs, the pulmonary artery will be raised, and the situation of the *ductus arteriosus* so changed, as to render the passage of the blood through it less favourable. Further, after birth, when the umbilical arteries are tied, the blood passing through the *ductus arteriosus* into the *aorta* will meet with greater resistance than formerly; since the fluids pushed into this artery by the two ventricles of the heart, will find a less ready passage, as a considerable part of the vessels through which they used to flow are thus obstructed.

HENCE, if respiration be once begun, though performed but for a short time, the blood, notwithstanding its being afterwards restrained

* M. de Haller (a) has well observed, that notwithstanding Harvey's words may seem to insinuate that a *fœtus* which has once breathed, instantly dies after this action is interrupted; yet *fœtuses* which have breathed only for a short time, can live several minutes without respiration.

(a) Element. Physiolog. tom. 3.

strained, will continue its course chiefly through the lungs, though, in that way, it cannot pass near so fast as it is thrown into those vessels from the right ventricle of the heart; whence it follows, that an animal, having been once used to breathe, and after this happening to be deprived of air, must be soon suffocated by an accumulation of blood in the pulmonary vessels *.

SOME

* As there have been some who doubted of the alternate motion of the lungs in respiration being necessary to carry on the circulation of the blood through their vessels, I shall here mention a few experiments, which seem to decide this point.

1. When the lungs are collapsed, *i. e.* in a state of expiration, as is the case of all dead animals, any fluid injected into the pulmonary artery passes with difficulty to the left ventricle of the heart.

2. When the lungs are inflated with air, an injected liquor flows through their vessels more easily and in greater quantity.

3. When the lungs are agitated with an alternate motion, something like natural respiration, water or any other thin fluid passes still more freely through them, and penetrates into their smallest vessels (*a*).

4. If respiration be restrained for any considerable time, one's face becomes of a purple colour, and its veins are much distended with blood; which shews that this fluid, on account of its difficult passage through the lungs, is accumulated in the trunks of the *cava* and right *sinus venosus*.

5. The necessity of the alternate motion of respiration, in order to the free transmission of the blood through the pulmonary vessels, is still more evidently demonstrated by the following experiment of Dr Musgrave: A dog whose *trachea* was cut, just below the *pomum Adami*, and close stopt with a cork, after a few violent struggles, died in two minutes; and, upon opening the *thorax*, the pulmonary artery, right ventricle and auricle of the heart, together with the great trunks of the *cava*, were distended with blood to a great degree; while the pulmonary veins, left auricle and ventricle of the heart, were almost quite empty, not containing more than a spoonful of blood (*b*).

It may be thought perhaps that the force of this experiment is weakened by one of Dr Hook's, who having cut away the ribs, diaphragm, and *pericardium* of a dog, and pricked the outer coat of the lungs with a penknife, preserved him alive, by keeping his lungs fully distended with a continued blast of air, which he made to pass through them by means of a pair of double bellows (*c*.) But as the blood flows much more freely through the pulmonary vessels when the lungs are inflated, than when they are collapsed, it is not surprising, that in this dog, which had lost a great deal of blood during a former experiment, the inflated lungs should afford an easy enough passage to the small quantity that would be thrown into their vessels by the contraction of the right ventricle of the heart; especially if we consider, that the constant stream of air must, while it was passing through the lungs, and escaping by the

(*a*) Vid. *Kaen Perspirat.* Hippocrat. dicta, sect. 160. 161. 162. & 170.

(*b*) *Philosophical Transact.* abridged, vol. 3. p. 67.

(*c*) *Ibid.* p. 66.

SOME may perhaps think the sudden death of animals deprived of the benefit of respiration, is owing rather to the want of something in the air which supports the vital flame. But without entering into a discussion of those arguments which may be brought for and against this opinion, I shall only observe, that since a *fœtus* can live a considerable time without respiration, when separated from its mother and involved in its secundines, its dying sooner for want of air, after it has once breathed, cannot be owing solely to the defect of any thing which this fluid might communicate to the blood in the lungs, but must be deduced from the change made in the pulmonary vessels by respiration, as has been above explained. And this reasoning seems to be confirmed from the observation, that animals through whose lungs a small share of the blood circulates, can sustain the want of air much longer than man and the other more perfect animals, in which the whole mass of blood passes through the pulmonary vessels: as likewise from this other observation, that new-born animals, which have breathed only for a short time, die not so soon in the air-pump as others do *.

HAVING thus shewn, that all the vital and other involuntary motions of animals are owing to *stimuli* of one kind or another, acting either immediately upon the organs moved, or on some neighbouring part with which they seem to have a peculiar sym-

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pathy;

the small wounds made in their external surface, have communicated a considerable oscillatory motion to all their vesicles and vessels, whereby the motion of the blood through them would be greatly promoted. And that a very small agitation of the lungs may be sufficient to keep up the circulation through their vessels, and preserve life, appears from the faintings which hysterical people are sometimes subject to. In these faintings, which I have seen last from 5 to near 15 minutes, the pulse beats a little slower and feebler than before, but with its usual regularity; while in the mean time there is no motion of the *thorax* observeable to the eye; however, by holding a lighted candle near the mouth, one can discover that they breathe, though it be weakly and slowly. Further, in Dr Hook's experiment, the passage of the blood through the pulmonary vessels must have been much freer than it is in sound animals, whose lungs are kept in a state of full inspiration, because they are, while in that state, considerably pressed upon by the rarified air endeavouring to inflate them more and more on the one hand, and the sides of the *thorax* resisting this inflation on the other.

* Philosophical Transact. abridged, vol. 2. p. 217. and 218.

pathy; it remains that we next inquire, whence this power of *stimuli* over the muscles of animals must be derived?

S E C T. X.

Of the reason why the muscles of animals are excited into contraction by stimuli.

THE muscular fibres of animals are so framed, as to contract whenever a cause proper to excite their action is applied to them, or, in defect of this, always to remain at rest. This cause is either an effort of the will *, or a *stimulus* of some kind or another †: to the former are owing the voluntary motions; and to the latter all such as we call vital and spontaneous.

How, or in what manner the will acts upon the voluntary muscles, so as to bring them into contraction, is a question beyond the reach of our faculties; and, indeed, were it otherwise, the answer would be here of no great importance, it being sufficient that experience convinces us that the will is really possessed of that power. But, in this our endeavour to trace the vital and other involuntary motions up to their first source, it seems to be a matter of no small moment, to investigate the cause or causes which enable *stimuli* of various kinds to excite the muscles of living animals into contraction.

1. HERE some have contented themselves with ascribing the contractions of muscles consequent upon pricking, tearing, stretching, or otherwise stimulating them, either by the application of solid bodies or acrid fluids, to the elastic power of their fibres ‡; but without informing us, whether by this they only understood that remarkable power of resiliency belonging to many bodies, and from which they are named elastic, or something different from, or superadded to this. But those authors would have done well to consider,

* Sect. 1. No. 7. above.

† Ibid. No 8.

‡ Bagliv. opera, 4to, lib. de fibra motrice, cap. 11. p. 335. et Dissert. de anat. fibrar. et motu muscul. p. 403. F. Hoffman. System. med. lib. 1. sect. 1. cap. 3. No. 17. et 18.

sider, that an elastic body, of whatever kind it may be supposed, is no more than a piece of dead inactive matter, without any power of generating motion; and that though it always recoils with a force proportional to that which bent or wound it up, yet it does this only in consequence of its being acted upon, and not from any agency of its own: That the sharpest needle does not produce stronger vibrations in the spring of a watch, than a blunt one acting upon it with equal force; and that spirit of wine, or oil of vitriol, dropt upon the most elastic body, disturbs not its state of rest, any more than the mildest milk, or oil of almonds. But the contrary of all this is true with respect to the action either of stimulating solid bodies or acrid fluids upon the muscular fibres of animals; whence it follows, that the motions they produce are not to be explained by any elastic powers with which, it may be imagined, these fibres are endued.

2. OTHERS, giving more scope to their imagination, have fancied the animal spirits lodged in the cavities of the muscular fibres, to consist of a number of little springs wound up, which, by the application of stimulating bodies, being put into vibratory motions, dilate these fibres, and so render the whole muscle shorter *. Not much different from this is the opinion of an illustrious author, who observes, in his excellent treatise upon the heart †, that this muscle is brought into contraction by the returning venous blood, which dilates its ventricles, and stretches their constituent fibres in such a manner, as to excite an oscillation in the animal spirits lodged in them, and consequently to make the muscular substance of the heart swell and become hard.

BUT, waving the objection, that as we are intirely ignorant of the nature of the animal or vital spirits, as they are called, every account of muscular motion from a *stimulus* which depends on their peculiar energy or manner of action, must therefore be merely hy-

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pothetical

* Lieutaud. Element. physiolog. p. 71. 72. et 261.

† Vol. 1. lib. 2. cap. 8.

pothetical and precarious ; may it not well be asked, if muscular contraction from a *stimulus* is owing to the animal spirits excited into an oscillatory motion, why should pressing the belly of a muscle with a smooth body produce a weaker oscillation than pricking it with a pin which is applied with less force, and affects only a very few of its fibres ? or why should one and the same irritating cause acting on the fibres of an inflamed muscle, raise a more violent oscillation of the nervous fluid, than when applied to a muscle in a sound state ? Besides, supposing the animal spirits lodged in the muscular fibres to be ever so elastic, would it not be in vain to attempt to deduce the motions of muscles consequent upon a *stimulus* from this property, since elastic bodies, as was observed above, never, of themselves, generate motion, but recoil only with a force proportional to that wherewith they are acted upon ? To alledge, that the animal spirits differ from other elastic bodies, or owe their oscillatory motion to some other cause *, will not be satisfactory ; for these spirits must either act intirely as a mechanical power, or not : if the affirmative be taken, it must be granted, at the same time, that their re-action, like that of other elastic bodies, cannot exceed the power acting upon them and putting them in motion ; but if their action, instead of being properly mechanical, be ascribed to some unknown active properties, this will be found, as I shall afterwards shew, an *hypothesis* inconsistent with the appearances of muscular contraction from *stimuli*.

3. IT may be thought, that muscular contraction is owing to some kind of explosion, ebullition, or effervescence, occasioned by the mixture of the nervous and arterial fluids, or perhaps to the agency of some subtile ethereal or electrical matter residing in the nerves ; and that as these causes may be brought into action by the power of the will, in order to voluntary motion, so, in the case of involuntary motion, they may be determined to exert their influence,

* Senac. Traité du coeur, vol. 1. p. 452.

fluence, by the mechanical action of heat, sharp instruments, or other *stimuli* applied to the fibres or nerves of the muscles *.

BUT, without inquiring how far the contraction or intumescence of a muscle may be owing, or not, to any of the causes now mentioned, it will not be difficult to shew that they cannot, without the intervention of some other agent, be excited to exert themselves by the various *stimuli* which are observed to bring the muscles of animals into contraction: for a fluid lodged in the nerves or muscular fibres, though of a nature fit to produce explosions, effervescences, &c. is not sufficient for any of these purposes, unless a cause peculiarly adapted to excite such motions be applied to it. Thus gun-powder produces no explosion without the assistance of fire; nor are electrical *effluvia* excited into action, but by the attrition of certain bodies. Alkalies then only raise a commotion when mixed with acids; and no effervescences or sudden ebullitions can be produced, without the mixture of substances disagreeing in their qualities. Fire applied to a glass globe will not produce electricity, any more than friction will make an alkaline liquor effervesce, or the mixture of an acid set gun-powder in a flame. If therefore muscular motion were owing to any of the causes above mentioned, it might reasonably be expected that it would only follow upon the application of certain kinds of *stimuli* to the muscular fibres: but we know from experience, that instruments of different metals, provided their sharpness and figure be the same, have an equal

* Dr Robinson has ascribed muscular contraction from heat, punctures, &c. to their exciting a vibrating motion in the *æther* within the nerves and membranes of the muscles; and thinks that the explosion of the electrical vapour brings the muscles into a strong and sudden contraction, by raising a strong vibrating motion in the *æther* lodged in their nerves and membranes; Animal œconomy, prop. 8.; and Dissertation on Sir Isaac Newton's *æther*, appendix, p. 140. Dr Langrish also is of opinion, that warmth and pricking with a needle renew the contraction of the heart, by putting in motion the ethereal matter of the nerves; Cronean Lectures, sect. 127. and 151. And as of late years there has appeared a fondness in some, to explain almost every hidden operation in nature by electricity, I thought it might not be improper to shew, that the electrical *aura*, even supposing it were the MATERIAL cause of muscular contraction, will not enable us to account for the motions of muscles, whose fibres or membranes are pricked, torn, or otherwise stimulated.

equal power of bringing the muscles of animals into action: that it makes no odds whether the stimulating substances be electrics *per se*, or non-electrics: that acrid liquors of quite opposite natures have much the same effect, if their degree of pungency be equal: that acids, alcalies, neutral salts, heat, pricking, tearing, and in short every kind of irritation, excite muscles of animals into contraction; and that there is no difference in the motions they produce, except what arises from their acting as stronger or weaker *stimuli*, *i. e.* from their irritating the part more or less.

FURTHER, no violent motion is produced by any bodies, however active, unless the peculiar causes necessary to produce such motion be applied to them: but in order to the contraction of a muscle, it is not necessary that the *stimulus* should be applied to its fibres; it is enough that the common membranes covering them are irritated, the same effect being thereby produced as from wounding the very fibres of the muscle. This is evident, in the case of the heart, stomach, intestines, and bladder; nay, many times, muscles are excited into action by a *stimulus* affecting a remote part with which they have no immediate connection, or so much as a communication by means of nerves, unless it be that general one subsisting between all the parts, as having their nerves derived from the same brain. Thus any thing which affects the internal membrane of the stomach after a disagreeable manner, brings the diaphragm and abdominal muscles into convulsive contractions: the action of light, as a *stimulus*, upon the tender *retina*, is followed by the contraction of the orbicular muscle of the *uvea*, and according to the various impressions made by sounds upon the auditory nerves, the muscles of the internal ear are variously contracted.

LASTLY, As the electrical *effluvia*, excited by the friction of certain bodies, are not emitted by fits and starts, but in a continued equable stream, so neither do the explosions or effervescences produced by the mixture of substances of disagreeing natures exert themselves, like irritated muscles, by alternate efforts. As little will the oscillations of an elastic *æther* (supposing the animal spirits
to

to be of that nature) serve to explain this appearance, since these must follow the laws of vibration observed in other elastic bodies, which yet are inconsistent, as we shall have an opportunity to prove below, with the alternate and vibratory-like contractions of muscular fibres occasioned by irritation. Upon the whole, then, we may conclude that the contraction of an irritated muscle cannot be owing to any effervescence, explosion, ethereal oscillation, or electrical power excited in its fibres or membranes, by the mechanical action of *stimuli* upon them.

4. SOME physiological writers have supposed a latent power or property in the muscular fibres of animals, to which their motions, in consequence of an irritation, are to be referred *.

BUT this opinion seems to have been too hastily embraced upon their despairing of success in their inquiries into this matter. For, if they here mean some unknown active powers resulting from the peculiar constitution or mechanical structure of a muscular fibre, it may be sufficient for us to deny that there are any such latent causes, as the asserters of them have been as unable to prove their existence by appearances which cannot be explained without them, as to specify their true nature; besides, it seems to be improper to attribute active powers to that, which, however modified or arranged, is yet no more than a system of mere matter? powers, I say, which are not only confessedly beyond those of mechanism, but seemingly contrary to all the known properties of matter.

FURTHER, the influence of *stimuli* in exciting even those muscles, to which they are not applied, into contraction, plainly argues such motion not to arise from any hidden power in the muscle being called into action by the mechanical effect or operation of the *stimulus*. Every attempt, therefore, towards explaining the motions of irritated muscles, from properties which their fibres, considered

* Peyer. Parerg. anatom. 7. p. 198.

A celebrated author, since the first edition of this Essay was published, has endeavoured to shew that the motions of irritated muscles are owing to the glutinous matter connecting those earthy particles of which their fibres are composed; and supposes irritability to be a particular property of that glutinous substance, in like manner as gravity is a property of matter in general. This hypothesis I have considered in my Physiological Essays, edit. 3. p. 179.—185. & p. 311. & 312.

as mechanical instruments, howsoever exquisitely framed, or nicely adjusted, can be supposed endued with, must be vain and fruitless: for as well might we pretend that the eye sees objects, and the ear hears sounds, purely by virtue of their being material organs, as imagine the motions of animal fibres from a *stimulus*, to be owing solely to their mechanical structure, or to the peculiar disposition of their parts.

5. SOME may be of opinion, that the all-wise AUTHOR of nature hath endued the muscular fibres of animals with certain active powers, far superior to those of common matter, and that to these the motions of irritated muscles are owing. And indeed we cannot but acknowledge, that he has animated all the muscles and fibres of animals, with an active sentient PRINCIPLE united to their bodies, and that, to the agency of this PRINCIPLE are owing the contractions of stimulated muscles. But if it be imagined that he has given to animal fibres a power of sensation, and of generating motion, without superadding or uniting to them an active PRINCIPLE, as the SUBJECT and CAUSE of these, we presume to say, that a supposition of this kind ought not to be admitted; since, to suppose that matter may, of itself, by any modification of its parts, be rendered capable of sensation, or of generating motion, seems to be as unreasonable as to ascribe to it a power of thinking. Matter, as far as we can judge by its known properties, appears to be incapable either of sensation or thought: and the whole appearances of the mere material world shew, that it acts invariably according to laws prescribed to it, and without any feeling, inclination, or choice of its own; nor is there any thing resembling will, self-determination, or real active power in the most refined and subtle parts of matter, more than in the most gross and sluggish.

IF then the effects of *stimuli* upon the muscular fibres of animals, cannot be deduced from any property or powers belonging to them, as mere material organs, it remains, that they are owing to an active sentient PRINCIPLE animating those fibres. But this will more evidently appear from the following considerations.

I. STIMULI

1. STIMULI applied to the muscles of animals, when laid bare, instead of only one contraction lasting for a considerable time, produce several contractions and relaxations alternately succeeding each other, which become gradually weaker, and are repeated after longer intervals, as the force of the irritating cause is diminished*. Now, these alternate contractions are easily accounted for, if we suppose them to proceed from a sentient PRINCIPLE, which, in order to the getting rid of the pain or uneasy sensation that arises from the irritation of the muscle, determines the influence of the nerves into its fibres more strongly than usual. For, if by one or two contractions the irritating cause be thrown off, and, with it, the disagreeable sensation removed, the muscle will return to its former state of rest; if otherwise, it will continue for a longer time to be agitated by alternate convulsive motions, which will be more or less forcible, and repeated after shorter or longer intervals, in proportion as the *stimulus* and painful sensation hence ensuing are stronger or weaker. The titillation of a slighter *stimulus* will be so much weakened by the first contraction of the muscle, that some space of time must intervene before it will be able to produce a second: whereas the smart pain which follows a strong irritation, affects the sentient principle so powerfully, that no sooner is the muscle relaxed, than a new contraction succeeds. Thus a gentle irritation of the left orifice of the stomach occasions only a slighter hiccup or convulsive contraction of the diaphragm, which, too, is not repeated till after considerable pauses; while a greater irritation, not only excites stronger convulsions of this muscle, but also a quicker repetition of them.

WHY the sentient principle, in consequence of a painful sensation, does not keep such muscles as are irritated in a continued state of contraction, but suffers them to be alternately relaxed, shall be afterwards explained.

IF the contraction of an irritated muscle were owing to the action of the *stimulus* upon it as a mere mechanical organ, then, so long as the *stimulus* continued to act equably, the muscle ought to remain equally contracted, and, upon its ceasing, the muscle ought to be re-

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laxed;

* Sect. 1. No. 10. and 11. above.

laxed; or rather the muscle, upon the first application of the *stimulus*, ought to be suddenly contracted; which contraction should become weaker by slow degrees, till at length the muscle has returned to its natural state of relaxation. If a few drops of any acrid liquor let fall on a bare muscle, or pricking it with a needle, excites it into contraction, as a mechanical cause acting upon a mechanical organ; then, so long as the cause acts on the organ, the effect must continue to follow; and if the cause becomes gradually weaker, so also must the effect, till it ceases altogether, *i. e.* the muscle ought not to be agitated with alternate convulsive motions; but, after its first and strongest degree of contraction, it should begin to lose some of its force, and continue to do so, till it returned again to its natural state.

WHAT most resembles muscular contraction from an irritation, is the falling or closing of the leaves of the sensitive plant after being touched: but this equally happens, whether these leaves be touched with the sharp point of a penknife, or the blunt end of a pencil, with a piece of smooth wax or rough iron, with brandy or with water. Here, there are no alternate contractions and relaxations, as in the muscular fibres of animals; no indication of feeling or of being peculiarly affected by stimulating substances; but all is affected by mere contact or mechanical impulse. I cannot help observing in this place, though foreign to my present purpose, that the closing of the leaves of the sensitive plant upon their being touched, cannot be owing, as some have imagined, to the electrical matter issuing from them; since the touch of wax, which repels this matter, makes them close as remarkably as that of non-electric bodies: nay, a piece of wax strongly electrified by rubbing, made the leaves of this plant quickly close, by attracting them.

IF it be said, that the elastic fibres of the muscles, or the nervous fluid supposed to be contained in their cavities, are excited by *stimuli* into strong oscillations, which are repeated till the irritation ceases, or even for some time after; I answer,

“THAT it is not easy to conceive how such *stimuli* as do not act by any mechanical force, but merely by their acrimony, should excite

cite an oscillatory motion in the supposed elastic fibres of the muscles, or in the animal spirits lodged in them.

¹ IF the motion of our muscles from a *stimulus* were owing to elastic vibrations of any kind whatever; how could the *sphincter pupillæ* and the muscles of the internal ear, continue uniformly and equally contracted for some time, which they never fail to do, when the *stimuli* affecting them act with unvaried force? And why ought they not rather to be agitated by a number of quickly repeated contractions? Nay, the continued, uniform, and equal contraction of the voluntary muscles would be impossible, if their motion was owing to any elastic oscillations.

² IF muscular motion from a *stimulus* were the effect either of the vibrations of the nervous fluid or of the solid elastic fibres of the muscles themselves, the alternate contractions of an irritated muscle, like the vibrations of elastic bodies, ought to follow one another at equal intervals, nor would they be more slowly repeated as they become weaker, and are about to cease; which however is the case. A musical chord, a bell, or any other elastic body, performs its vibrations in equal times, whether it be acted upon by a stronger or a weaker force; and its oscillations follow one another, from first to last, with an equal degree of swiftness: In like manner the elastic pulses which these bodies communicate to the ambient air, succeed each other as quickly in distant places, where the sound is faint, as in those near the sonorous body, where it is stronger. Since therefore the alternate contractions of irritated muscles do not follow the law of the vibration of elastic bodies, but become remarkably slower when they decrease in strength, and before they cease altogether; it follows that they cannot be owing to any elastic vibrations excited in the muscular fibres, or in the nervous fluid contained in them. But of this more fully afterwards *.

2. IF it were constantly observed, that such muscles only as had their fibres immediately acted upon by *stimuli*, were excited into contraction,

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* Vid. sect. xiv. below, on the motion of the muscles of animals when separated from the body.

traction, then indeed it might be suspected, that such motions were no more than a necessary consequence of the mechanical action of those *stimuli* upon the muscular fibres: but as we find the muscles of animals brought into action without any irritation of their fibres, whenever a *stimulus* is applied to the coats or membranes covering them, to the nerves which are sent to them, or to some neighbouring or even distant part, it seems unreasonable to ascribe such motion to the mechanical action of the *stimulus* upon the fibres of the muscle, and not to the impression it makes on the sentient principle. Thus the contraction of the *sphincter pupillæ* arising from the action of light on the *retina*, with which it has no communication of nerves, cannot be explained mechanically, but must be owing to some sentient principle in the brain, which, excited by the uneasy sensation, increases the action of the nervous power upon that muscle. The same thing is also true of the various motions of the muscles of the *malleus* and *stapes* from different sounds striking upon the auditory nerve; and of the motions of the eye-lids as often as any thing irritates the *cornea*, be it ever so gently. The contraction of the diaphragm and intercostal muscles, in consequence of an uneasy sensation in the lungs, must also be owing to the mind or sentient principle acting at the origin of the nerves, and not to any change wrought mechanically upon the fibres of these muscles, by the difficult passage of the blood through the pulmonary vessels. The violent action of the diaphragm and abdominal muscles in a *tenesmus* or stranguary is to be explained in the same way. If a spark from the fire, or a drop of boiling water falls upon one's foot, the leg is instantly drawn in towards the body; but as the muscles employed in this action are those which run along the thigh, and are inserted about the head of the *tibia*, it is manifest that this *stimulus* cannot excite those muscles into contraction in consequence of any mechanical action upon them; and if the sympathy of the nerves, or continuation of membranes, shall be assigned as the cause of this motion, it may be justly asked, why the muscles which run along the leg, and are inserted into the foot, are not more moved than those of the thigh, since they have a nearer connection with that
part

part to which the *stimulus* is applied; or why the extensors of the leg are not brought equally into action with its flexors? It remains therefore that the motion of the leg, in this case, be attributed to the pain or uneasy sensation excited by the fire or boiling water, for avoiding of which the sentient principle is instantaneously determined to put the flexors of the leg in motion, and so to remove the member from the offending cause. Nay, where the *stimulus* is applied to the membranes or teguments of the muscles, it seems probable, that the subsequent contractions are not owing to any change first made on their fibres: thus the convulsive motions of the intercostal and other muscles of the trunk of the body, which are excited by tickling the sides, must be ascribed to the mind, which, in order to avoid the disagreeable titillation, puts these muscles in action, and not to any immediate influence which the tickling can have on their fibres; otherwise why should the same mechanical action of our own fingers, and those of another person, affect us so differently? Tincture of *ipecacuanha* applied to the internal surface of the stomach, does not seem to produce the convulsive contractions of that organ in vomiting, by immediately affecting its muscular coat, which is defended by the nervous and villous ones, but by irritating its nervous *papillæ*, and thereby affecting the mind or sentient principle.

SINCE, therefore, *stimuli* applied, not only to remote parts, but also to the membranes or coats immediately covering any muscle, excite it into contraction by the intervention of the mind; is it not reasonable to think, that even when the muscles themselves, or a few of their fibres, are irritated, the subsequent motions are owing to the mind's being excited, by a disagreeable sensation, to determine the influence of the nerves more strongly into them?

3. THIS will further appear, if we consider that not only an irritation of the muscles of animals, or parts nearly connected with them, is followed by convulsive motions; but that the remembrance or idea of substances, formerly applied to different parts of the body, produces almost the same effect as if those substances themselves were really present. Thus the sight, or even the recalled idea of
grateful

grateful food, causes the *saliva* to flow into the mouth of an hungry person ; and the seeing a lemon cut produces the same effect in many people. The sight of an emetic, nay its very name, or seeing others vomit, will in many delicate persons raise a *nausea*. The apprehension or fear of having one's sides tickled, causes almost the same motions in the trunk of the body, as tickling itself would do, though in a less degree.

FURTHER, That many remarkable changes and involuntary motions are suddenly produced in the body by the various affections of the mind, is well known. Thus fear often brings on a sudden flow of pale urine. Sore eyes have been believed to be infectious to those who look at them. Certain sounds occasion a *tremor* over the whole body. The sound of a bagpipe has been said to give some people an inclination to make water. The sudden appearance of any frightful object, will, in delicate people, cause a palpitation. The sight of an epileptic person in the fit has brought on an epilepsy ; and yawning is so very catching, as frequently to go round a whole company. In these cases, the motions produced in the vessels of the eyes or eyelids, in the heart, stomach, and bladder, in the secretory vessels of the salivary glands and kidneys, in the muscles employed in yawning, &c. cannot be owing to the mechanical action of the causes above mentioned upon the fibres of the parts moved: for what particular connection is there between the optic and auditory nerves, and those which serve the heart, stomach, bladder of urine, mouth, salivary glands, and the muscles which depress the lower jaw and move the trunk of the body? All the nerves do not at last terminate in a point, but in a large space of the brain ; therefore the consent between them cannot be deduced from their contiguity, but from a sentient principle, which is present, at least, wherever the nerves have their origin, and which, accordingly as it is variously affected, produces motions and changes in different parts of the body.

IF then external causes affecting the brain do, by the intervention of the mind or sentient principle, produce considerable changes in the muscles of spontaneous as well as in those of voluntary motion ; and, if the idea of a *stimulus* has, in many cases, almost the

the same effect as the thing itself; is it not reasonable to think, that stimulating substances applied to the muscles of animals excite them to contract, not by any immediate mechanical action upon their fibres, nor by producing an explosion or effervescence, nor by exciting strong vibrations in an ethereal or electrical matter lodged in these fibres or their nerves; but by disagreeably affecting the sentient principle, in consequence of which it increases the action of the nervous power upon the fibres of those muscles which are stimulated. And there seems to be the less difficulty in this doctrine, as the various appearances above mentioned would evince the presence, agency, and extensive influence of something in the bodies of animals, of a nature different from mere matter, and of powers superior to it, however modified, compounded, or arranged.

IF *stimuli* excite the muscles of animals into contraction by acting upon them, rather as sentient than mere mechanical or material organs, it is easy to see, why the slightest aliment is apt to occasion vomiting when the coats of the stomach are inflamed, and why the heart is agitated with violent convulsions and palpitations as often as itself, or even the *pericardium*, is affected with any degree of inflammation. In these cases the stomach and heart are rendered extremely sensible and impatient of any irritation; hence the *stimuli* which used to affect them gently, now excite them into violent convulsions.

IT was observed above, that those muscles to whose fibres *stimuli* are applied, do not remain contracted for any considerable time, but are agitated with alternate contractions and relaxations. Thus any of the muscles of the eye, by irritating their tendinous fibres with a sharp instrument, are made to beat almost like the heart of an animal affected with strong palpitations *. But in muscles whose

* A learned author seems to have fallen into a mistake with respect to this matter, when he observes, that an irritation makes the muscles of living animals only perform one contraction, although the same cause produces many repeated contractions in the muscles of those that have been newly killed; for, besides the instance of the muscles of the eyes here mentioned, the heart of an animal is observed to be agitated with violent and quickly repeated convulsions when it is pricked with a sharp instrument immediately upon opening the *thorax*; and, if any other muscle of a living animal be laid bare, it will, by irritating its fibres or membranes,

whose contraction is owing to the action of a *stimulus* upon some distant or neighbouring part, there is a diversity observed; some of them being uniformly contracted while the irritating cause lasts, others alternately contracted and relaxed: thus the action of light and sound upon the *retina* and auditory nerves produces an equable constant contraction of the *sphincter pupillæ* and muscles of the internal ear; while an irritation of the membrane of the nose and *trachea* is followed by alternate convulsive motions of the muscles of respiration; and a titillation of the inferior extremity of the gullet, by repeated contractions and relaxations of the diaphragm.

THESE very different effects of *stimuli* on different muscles and organs of the body, which to me appear inexplicable upon mere mechanical principles, seem to be easily accounted for upon those which I have already laid down: for if the contraction of an irritated muscle be owing to the uneasy sensation excited by the *stimulus*, as often as the first contraction does not remove this, the muscle will be agitated with alternate convulsions, as being most proper to throw off the irritating cause. If indeed, by the first contraction, the disagreeable sense of irritation be quite removed, no further motion follows; but if it still remains, new convulsive contractions will succeed, and continue alternately, till the *stimulus* either ceases intirely, or becomes too weak to produce a new contraction. But when the contraction of any muscle occasioned by the action of a *stimulus* on a neighbouring part, would, if it was alternate, neither tend to remove the irritation, nor render the mind less sensible of it, in that case no sudden relaxation follows, but the muscle remains equally contracted as long as the stimulating cause continues

membranes, be brought into alternate contractions. A *stimulus* therefore applied to the muscles of animals excites them, if it be considerable, into alternate repeated contractions, whether the animals be alive or newly killed; only the convulsions in the former case are stronger than in the latter. Nay, it will be difficult to reconcile this supposed difference in the effect of an irritation on the muscles of living and newly killed animals with that principle which this learned author has laid down, namely, that the contraction of an irritated muscle is owing to the re-action of the animal spirits lodged in its fibres, in consequence of the action or impression of the *stimulus* upon them; for there appears to be no reason why the re-acting power of the animal spirits should continue to exert itself longer or stronger in the muscles of dead animals, than in those of living ones.

tinues the same. Let us now see how this principle can be applied to the different spontaneous motions of animals.

THE alternate contractions and relaxations of the muscles of respiration in sneezing, are most wisely adapted to remove the irritating cause from the membrane of the nose, and to lessen the uneasy sensation arising from it: if, by the air, first strongly inspired and immediately after more forcibly expelled through the nose, the *stimulus* affecting its nerves be removed, no new contraction ensues; if not, the action of sneezing is still repeated, till the titillation in the nose ceases, or becomes too weak to produce a new convulsion. In sneezing, inspiration is only performed in order to make way for the succeeding violent expiration, which most effectually removes the uneasy sensation or irritating cause; at the same time, the strong and sudden contraction of the inspiratory muscles acts, partly, as a kind of *stimulus* in exciting the subsequent convulsive motion of the expiratory ones. The alternate contractions of the diaphragm in the hiccup, and of the muscles of respiration in coughing, evidently tend to remove or lessen the uneasy sensation in the gullet and *trachea*, and are therefore not continued, but interrupted by alternate relaxations. On the other hand, a strong irritation of the *intestinum rectum*, from too great a quantity of excrement, produces a continued contraction of the abdominal muscles and diaphragm, because, in this case, the contraction of these muscles has no effect to lessen the uneasy sensation, till the *fæces* are expelled. It is true indeed, that when the body is bound, several efforts of the diaphragm and abdominal muscles are required before any of the excrement can be expelled; but the action of these muscles is, in this instance, interrupted, not on account of the irritation in the *rectum*, but in order to carry on respiration, which cannot long be suspended without occasioning an uneasy sense of suffocation in the lungs, by which we are more affected than by the *stimulus* of the *fæces*.

THE causes which produce the erection of the *penis* *, though they be generally excited into action by the *stimulus* of the *semen*,

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* See above, Sect. vi. No. 4.

yet do not act by alternate fits, because the erection has no immediate effect in lessening the stimulating cause: but the contraction of the *musculi ejaculatores seminis* is alternate, because by each convulsive motion, the *semen*, i. e. the irritating cause, is expelled. The orbicular muscle of the *uvea*, and the muscles of the *malleus* and *stapes*, remain equally contracted, while the same degree of light and sound is applied to the eye and ear, because their contraction does not hinder those causes from acting uniformly and equally upon the *retina* and auditory nerve; but no sooner is more or less light applied to the eye, or a stronger or weaker sound to the ear, than those muscles are more contracted, or somewhat relaxed. With respect to the heart, as the returning blood or irritating cause is alternately received into its cavities and expelled, it is easy to see why it should, like the *ejaculatores seminis*, be agitated with regular alternate contractions. And as, by the *systole* of every portion of the intestines, the air, aliments, &c. are pushed into the succeeding ones, the motion here must also be alternate; only not so equal and regular as in the heart, where the alternate action of the irritating cause is more uniform. On the other hand, the bladder in expelling the urine, acts not alternately, because the *stimulus* remains present with it, and an alternate motion of that organ would not have been so proper for the expulsion of urine as its continued contraction.

WHEN the fibres of a muscle are irritated, by wounding them with a sharp instrument or otherwise, a strong convulsive contraction instantly ensues; which is suddenly followed by a relaxation, because an uniform continued contraction would not be so proper to drive off the offending cause from the muscle, as alternate contractions and relaxations: and we are so framed by nature, as spontaneously, and without any previous reflection, to perform those motions and actions which tend most effectually to the preservation of our bodies. It is probable, however, that the alternate relaxations of irritated muscles may be owing to the uneasy sensation's being some way

way lessened by each contraction *, on account of which, the sentient PRINCIPLE, as being now less affected, immediately allows the muscle to be relaxed. This is the case in the hiccup, where the convulsions of the diaphragm weaken or suspend, for some little time, the sense of irritation in the inferior extremity of the gullet. The relaxations, however, of stimulated muscles do not last for any considerable time, but are quickly succeeded by new contractions, because the painful sensation soon begins again to affect the mind more strongly : but as the irritation becomes gradually weaker, these alternate contractions will not only grow feebler but succeed one another more slowly ; for while the irritation is strong, the muscle is no sooner relaxed, than its contraction is immediately renewed : whereas a weaker *stimulus* requires a longer time to operate, before it excites an uneasy sensation requisite for producing a convulsive contraction of the part. Thus when the *thorax* of a living animal is laid open, and the heart is pricked with a sharp instrument, its contractions are much quickened ; nay, they return so frequently, that, during their remission, very little blood can enter the ventricles. Hence the sides of the heart make very small motions at first, nor are they ever fully dilated, their contractions being repeated almost as soon as their *diastole* begins ; but when the impression of the *stimulus* begins to be considerably weakened, the contractions and relaxations of the heart being performed more slowly, the blood has time to dilate the ventricles more, whose sides, therefore, now make larger and more sensible motions. From what has been said, it is easy to see, why, if the blood be rendered acrid, or the heart more irritable than usual, the pulse becomes small, fluttering, and quick.

WHILE therefore the voluntary muscles (which are contracted

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* If any one doubts that the disagreeable sensation, excited by the irritation of a muscle, will be less sensibly perceived during its contraction, let him consider that brutes, by the motion of their *panniculus carnosus*, not only drive off flies and other insects which annoy them, but, by the very action of this muscle, they seem to become less sensible of the tickling. That friction of a part lessens any itching or painful sensation in it ; and people when uneasy, often change postures, because, during the motion of the parts necessary to make this change, they are less sensible of the uneasiness.

in consequence of an effort of the will) remain contracted as long as the will continues to determine the influence of the nerves into their fibres, the irritated muscles (whose contraction is owing, not to will or choice, but to an uneasy sensation) are entirely regulated by that sensation; and as each contraction tends to lessen the disagreeable perception, they will be agitated with alternate convulsions.

UPON the whole, as nature never multiplies causes in vain, it seems unnecessary, in accounting for the motions of the muscles of animals from *stimuli*, to have recourse to any hidden property of their fibres, peculiar activity of the nervous fluid, or other unknown cause, when they are so easily and naturally explained, from the power and agency of a known sentient PRINCIPLE.

S E C T. XI.

Of the share which the mind has in producing the vital and other involuntary motions of animals.

THAT all the motions of animals were by some of the ancient philosophers ascribed to the energy of a living principle wholly distinct from the body, the passage of Cicero prefixed to this Essay clearly shews. And it was the difficulty of accounting for the motion of the heart from mechanical principles alone, which made Borelli doubt, whether it were not rather owing to the mind, than to any natural necessity arising from the structure of that organ or its nerves*. The celebrated Leibnitz, in a letter to Michelloti, goes still further, and supposes that the natural motions may be owing to some impressions made on the mind, altho' we are no ways conscious of them †. It is true that Stahl, by extending the influence of the soul,

* De motu animal. part 2. prop. 79. & 80.

† Michellot. de separatione fluidor. p. 351.

Indeed, strictly speaking, it is inconsistent with Leibnitz's principles to ascribe any of our motions, either voluntary or involuntary, to the mind; since, according to his *pre-established harmony*, the soul neither acts on the body, nor is affected by the impressions of external objects. This ingenious fable, however, has been so fully refuted by Bayle and others, as to make it needless for us to attempt to shew its weakness.

soul, as a rational agent, over the body a great deal too far, has been the occasion why, for many years, it has been considered rather as a subject of ridicule, than deserving a serious answer. However, that the motion of the heart and circulation of the blood, are inexplicable upon principles purely mechanical, there are arguments *a priori* which seem to evince*. But, as this kind of reasoning, in matters which do not admit of strict demonstration, frequently betrays us into error, we shall endeavour further to vindicate this opinion from the most plausible objections which may be brought against it; and likewise shew by some arguments, *a posteriori*, chiefly of the analogical kind, that the vital, as well as the other involuntary motions of animals, are directly owing to the immediate agency of the mind or sentient principle.

THE chief power propelling the blood through all the vessels of the body, is the contraction of the heart. But from the Reverend Dr Hales's experiments it appears, that in every circulation, the blood loses $\frac{2}{10}$ of the *momentum* communicated to it by the left ventricle of the heart†; therefore there must be in every animal some cause which repairs this loss of motion arising from friction, &c. *i. e.* a cause generating motion: But, as has been observed above, matter, in its own nature inert, is incapable of this. Further, such animals as lie in a dead inactive state during the winter, and whose blood has lost its motion, may at any time be restored to life by warmth, which, rarifying the stagnating fluids, and communicating to them a small degree of intestine motion, excites the heart into action; this motion, at first languid, gradually gains strength, till at last it arrives at its wonted vigour. As, in this case, the renewal of the heart's motion, and its gradual increase, cannot be explained from any mechanical principles, (since we have not only a cause producing an effect greater than itself, but also an effect increasing by degrees, and, as it were, of its own accord), it follows,

* See Edinburgh Medical Essays, vol. 4. art. 14. where a clear and strong argument of this kind is proposed by the learned Dr Porterfield.

† Statical Essays, vol. 2.

lows, that there is in these animals some living principle, which, by the *stimulus* of warmth, being roused, as we may say, out of a state of indolence, brings into gentle contractions the *sinus venosi*, auricles and ventricles of the heart; *viz.* those parts of the body most sensible of the irritation of the fluids when rarified and agitated by heat.

THE contraction of the heart, so far as it is owing to a material cause, seems to proceed from the action of the nervous power upon its fibres: but as, perhaps, $\frac{1}{100000}$ part of the blood thrown out by the left ventricle of the heart, does not return to it again in the form of vital spirits, as they are called; and as the motion of this fluid must be much diminished by its passage through the very subtle vessels of the *cerebellum*, &c. there can be no force in these spirits derived from the last *systole* of the heart sufficient to produce a new contraction of that muscle, since no cause can generate an effect greater than itself.

AGAIN, The human body, in which there is no mover that can properly be called FIRST, or whose motion depends not on something else, is a system far superior to mechanism. The contraction of the heart is indeed the cause of the motion of the blood, and consequently of the secretion of the spirits (as is supposed) in the *cerebellum*, &c.; but without these spirits, this action of the heart could not be performed: these two causes, therefore, truly act in a circle, and may be considered mutually as cause and effect. Hence it seems to be incumbent on those philosophers, who ascribe the motion of the heart to mechanical causes alone, to demonstrate the possibility of a *perpetuum mobile*, since, as long as life lasts, an animal is really such. But as a perpetual motion is, in the opinion of the ablest philosophers, above the powers of mechanism*, we must be allowed to conclude, that the contraction of the heart, and the propulsion of the blood through the body, and consequently the continuance of life, are not owing to any mechanical or even material

* "Ex calculo mechanico liquet omnem de motu perpetuo questionem eo redire, ut inveniatur pondus seipso ponderosius, vel vis elastica seipsa fortior." Clarkii not. in Røhault physic. § 1. cap. 22.

terial causes alone, but to the agency of a living principle capable of generating motion.

How far the mind is really concerned in the motion of the heart, may easily appear from what has been already offered in the preceding sections; where, if I mistake not, it has been shewn, that the contraction of the heart is owing to the returning venous blood acting as a *stimulus* upon it; and that a *stimulus* excites our muscles into motion, only as they are animated by a sentient principle. Hence it must follow, that the alternate contractions of the heart are in no other sense owing to the irritation of the returning blood, than as the mind or sentient principle is, by this, excited to increase the action of the nerves upon its fibres.

THIS doctrine of the alternate motion of the heart as proceeding from the power of the mind, excited into action by the *stimulus* of the returning venous blood admitted into its cavities, is strengthened by the account which we have given of the alternate motions of respiration, of the contractions of the muscles of the internal ear and of the pupil. These we have shewn to proceed from the mind, as affected by a *stimulus*, and to be inexplicable upon principles merely mechanical. The first of these motions (*viz.* respiration) agrees with that of the heart, in being performed whether we attend to it or no, and whether we sleep or are awake; although it differs from the motion of the heart, in being under the dominion of the will. The motions of the *iris* from light, and of the muscles of the ear from various sounds, differ from those of the heart, as they are not vital, nor continually and alternately excited by causes within the body, but owing to external causes acting at particular times on the organs of sight and hearing: These muscles, however, exactly agree with the heart in this, that their motions are involuntary, and cannot be controlled by any immediate effort of the will. Since then, in the muscles of respiration, we have an instance of a vital, though not altogether involuntary motion, proceeding from the mind; and, in the muscles of the *uvula* and ear, examples of motions, which, though not vital, are yet wholly involuntary, owing to the same cause; may we not conclude, that the contraction
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of the heart, which is both vital and involuntary, is ultimately to be referred to the sentient principle excited to put this muscle in motion by the irritation of the venous blood acting upon it alternately? What has been said of the motion of the heart, as proceeding from the mind, is equally applicable to the peristaltic motion of the stomach and intestines, and to the rest of the vital or involuntary motions.

ALTHOUGH what has been already offered, may to some be sufficient to shew, that all the motions of animals, involuntary as well as voluntary, are some way owing to the mind; yet as this doctrine may still be attended with difficulties to others, we shall endeavour briefly to obviate some of the strongest objections, which, at first sight, seem to lie against it; and this we shall the more willingly do, as an opportunity will thence arise of still further illustrating the nature and cause of the involuntary motions of animals.

Objection I. IT may be said, That, while we ascribe the vital and other involuntary motions of animals to the mind, we, in fact, attribute them to a power whose nature and manner of acting we are ignorant of *.

Answer. THAT there is united to the bodies of men and animals an active, living, sentient principle, which is the cause of voluntary motion, it may be hoped there are few philosophers at present who will deny: and, if it be thought no absurdity to ascribe voluntary motion to the energy of the mind, though we do not understand its nature or manner of operation, why should it be reckoned such, to derive the vital and other involuntary motions from the same source; especially, when a variety of appearances and analogy concur in supporting this opinion: That there is such a thing as gravity, or attraction betwixt the parts of matter, is a thing not to be doubted of, because we see its effects, though its cause be unknown: And, if philosophers are allowed to make constant use of this power, in order to explain the *phenomena* of nature, why should it not be thought equally reasonable to have recourse, in

* Senac Traité du cœur, vol. I. p. 441. et 445.

in accounting for the motions and actions of an animated body, to the power and agency of the mind, which we are sure is always present with it, and in numberless instances operates upon it? In accounting for some surprising operations of an inanimate machine, it would be thought very unphilosophical to have recourse to the agency of an immaterial living principle: and must it not be equally so, to banish the consideration of the mind in explaining the appearances of an animated system; or to endeavour to deduce its most principal motions from the mere material part?

THERE is no need of understanding the nature of the soul, or the way in which it acts upon the body, in order to know that the vital motions are owing to it: it is sufficient, if we know from experience, that it feels, is endued with sensation, and has a power of moving the body *.

EVERY attempt hitherto made towards deducing the vital motions of animals from powers wholly material, has been unsatisfactory, and I believe that I may venture to say, will be for ever vain: since I have endeavoured not only to prove, that they are superior to the force of mechanism, but also that the supposition of any mere material power's being their cause, is not suitable to the appearances. Nor can I conceive why physicians should have so long laboured in accounting for the action of the heart and other vital motions of animals, from the powers and properties of body independent of the mind, if it be not, that with some, the Cartesian principles still continue; with others, too great a fondness for mechanical reasoning in physiological matters prevail; and in both, a contempt of the indeed extravagant notions of Stahl and his followers, with regard to the manner in which the mind regulates all the actions of the body †.

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* The reader will easily perceive, that the objection against deriving the vital motions from the mind, because we are not fully acquainted with its nature and manner of acting, may be applied with double strength to those who ascribe the contraction of the heart to some latent power in it, or to the oscillations of an unknown fluid lodged in its fibres and nerves.

† I have met with no author, who has embraced the STAHLIAN doctrine with less reserve,
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MANY philosophers have supposed two distinct principles in man ; one of which has been called the *anima* or soul ; the other, the *animus*, or mind ; by the former, they understood the principle of life and sense influencing the vital motions ; and by the latter, the seat of reason or intelligence. According to them, we have the *anima*, or vital and sentient soul, in common with the brutes ; but the *animus*

or carried it to a greater height, than a learned physician, in his late elegant *Prælection de anima medica*. According to him, the soul at first forms the body, and governs it ever after, carrying on and regulating all its vital and natural motions ; distributing the fluids with greater or less force to its several parts, and exciting in them, from time to time, such commotions and changes as she sees most proper for removing their various disorders. He ascribes it to the prudence of the soul, that the *semen* is not perfected in males, till the body hath acquired strength and vigour sufficient for the work of generation : and he finds a strong instance of her sagacity, in the slow and gradual eruption of the small-pox ; as the force of the disease is thereby divided, and its danger much lessened. When the body is disordered or exhausted with fatigue, the soul frequently hides herself in sleep, and retires from external things, in order that she may be more at leisure to recruit the body, or to rectify what has happened amiss in it ; and hence the inclination to sleep after child-bearing : hence also the frequent sleeping of infants, whose *anima*, it seems, is so taken up with directing and governing the vital motions, that it has little time to attend to any thing else. The soul, however, seems to neglect, in a great measure, this province, as often as she is too much distracted with external things ; and hence it is, that health is so much impaired by fear, sorrow, love, and other strong passions ; nor is she without her wilful and forward fits, as appears from her sending the milk back into the blood from the breasts of pregnant women whose *fætuses* she had only fancied were suddenly dead, and from her not deriving into them again those nourishing streams, when living children are really born ; as if she had rather they were starved, than that she should seem to have been mistaken. In fevers, the sudden failure of the strength and pulse ought, we are informed, to be regarded as signs of the despairing soul's discontinuing her care of the body, and being soon about to relinquish it : nay, sometimes, like a coward, she sinks even under such diseases as, in their own nature, are not at all deadly ; and, through false alarms, she is either thrown into a great hurry and trepidation, which urges her to do much mischief ; or else she becomes backward and remiss in her endeavours to preserve the body, and, as if it were a field not worth keeping, foolishly deserts it : though, were she but always wise enough, and, neglecting things of less moment, were solely intent on the preservation of the body, she could, if we may believe the author, not only prevent diseases, as far at least as they proceed from internal causes, but protract also the life of man, it may be, to a thousand years : a term greatly beyond what the Adepts promised themselves from their *aurum potabile*, or universal remedy.

But, as as this account of the agency of the soul, and of its power over the body, scarcely seems to demand an answer, I shall only observe, that to imagine the soul should, with the wisest views and in the most skilful manner, at first form the body, (a work far above the efforts of human

animus or *mens*, which is of a more exalted nature, is proper to rational creatures alone *.

SOME modern materialists have imagined the *anima* to be no other than a more subtile kind of matter lodged chiefly in the brain and nerves, and circulating with the groffer fluids. But such spirits, or subtile matter, can no more be acknowledged the vital principle or source of animal life, than the blood from which they are derived; and still with less reason can this material *anima* be supposed endued with sense, since matter, of itself, and unactuated by any higher principle, is equally as incapable of sense or perception, pleasure or pain, as it is of self-motion. Indeed, a few authors have gone so far, as to suppose even the *animus*, or rational soul itself, material: but surely the powers and faculties of the mind are not to be found in matter, or in any of those principles, or elements, whereof either the ancients or moderns have imagined it to consist: fire itself, the most subtile and active among them, being as incapable of thought and reflection, as water or earth, which are the most sluggish †: and in what manner self-motion, sense, or reason can re-

T 2 sult human art and contrivance !), and afterwards, when it is disordered, should, with the same skill and wisdom, often remedy the evil, and restore it to a sound state; but finding it in the end, or sometimes suspecting it only, to be no longer tenable or comfortable, should, instead of repairing, either whimsically or wisely desert it: to conceive, I say, of the soul as performing all this, without, in the mean time, being conscious of such intentions, or of the exertions of its power in pursuance of them, is at least as fanciful as to suppose, that an architect might raise a stately edifice, in which nothing should be wanting that could contribute either to its usefulness or ornament; that he might frequently repair such damages as it sustains from the weather, or from the decay of any of its materials; and at last, apprehending it to be in danger of falling, might abandon it; without being conscious of ever having once exercised either his skill in contriving, erecting, and repairing it, or his prudence in quitting it, when, as he thought, it was ready to bury him in its ruins.

* ——— *Indulsi communis conditor illis*

Tantum animas, nobis animum quoque.

JUVENAL. Sat. 15. lin. 148. 149.

† “ *Animorum nulla in terris origo inveniri potest. Nihil enim est in animis mixtum atque concretum, aut quod ex terra natum atque fictum esse videatur: nihil ne ut humidum quidem, aut stabile aut igneum. His enim in naturis nihil inest, quod vim memoriæ, mentis, cogitationis habeat, quod et præterita teneat, et futura provideat, et complecti possit præsentia.*” Cicero in Tusculan. disput. lib. 1.

See also, in proof of the immateriality of the soul, Dr Sam. Clarke's defences of his letter to Mr Dodwell, where perspicuity, metaphysics, and sound philosophy, are happily united.

sult from the circulation of the spirits in the brain, or from the peculiar structure, connection, situation, or arrangement of the various parts of the body, (without supposing a mind), is a point which the abettors of materialism, I believe, will never be able to clear up *.

FURTHER, if the DEITY be incorporeal, (as is allowed by some of the ablest advocates for the materiality of the human soul), and if he can create intelligent beings that are not material, is it not agreeable to analogy to consider animals (which hold as it were the middle rank in the creation between purely spiritual intelligences and substances that are wholly corporeal) as deriving their sentient and rational powers, not from that inert matter, however curiously wrought up or modified, of which their brain and nerves are composed, but from an intelligent, incorporeal principle animating their bodies?

UPON the whole, as I cannot agree with those who, in ascribing all our powers to mere matter, seem willing to deprive us wholly of mind; so neither do I see any reason for multiplying principles of this kind in man: and, therefore, I am inclined to think that the *anima* and *animus*, as they have been termed, or the sentient and rational soul, are only one and the same principle acting in different capacities. Nay, Epicurus himself, according to Lucretius, did not look upon these two as separate beings, but considered the mind as a kind of *mouvement* produced by the *anima* or soul †.

THAT the involuntary motions in man are not owing to a principle distinct from the rational soul, seems probable, from the muscles and organs, whose action has been generally ascribed to the *anima*, being, in many cases, subject to the power of the *animus* or rational principle; as well as, on the other hand, from the motions of the voluntary muscles often becoming involuntary, or independent

* “Membrorum verò situs et figura corporis, vacans animo, quam possit harmoniam efficere non video.” Cicero. Tusculan. disput. lib. 1.

† Nunc *animum* atque *animam* dico conjuncta teneri
Inter se, atque *unam naturam* conficere ex se.

Lucret. lib. 3. vers. 137. & 238.

dent upon the will. Thus the diaphragm, whose motions in the hiccup are involuntary, and in ordinary respiration continue without our consciousness of them, is nevertheless subject to the immediate influence and direction of the mind; since its motions in breathing can, by an effort of the will, either be augmented or lessened, retarded or accelerated. The evacuation of the *intestinum rectum* and bladder of urine, which, when the *stimulus* is gentle, is in part voluntary, becomes altogether involuntary and convulsive, when the irritation is greater. The eye-lids, over which the mind seems to have a full power, move, commonly, not only without our attention, but, in some cases, even against every effort of the will to the contrary. The action of the *acceleratores urinæ* is voluntary in expelling the last drops of water; but in expelling the *semen*, it is involuntary. The contraction of the pupil, which, in order to distinct vision, is voluntary, becomes wholly involuntary when owing to an increase of light. In short, there is not a voluntary muscle in the body, whose motion does not become involuntary, as often as it is either directly, or from its consent with some neighbouring part, affected by any considerable *stimulus*: if the irritation be very gentle, we still retain a greater or less power over the muscle; but when it becomes stronger, we lose all this power.

FURTHER, in man the sentient and rational principle must be acknowledged to be one; since we are all conscious that what feels, reasons, and exerts itself in moving the body, is one and the same, and not distinct beings. It is the mind, therefore, that feels, thinks, remembers, and reasons; which, though one principle, is nevertheless possessed of these different powers, and acts in these different capacities: nay, since memory is as different from the present perception of ideas, or the exertion of the will in order to action, as sense is from reason, it might with equal propriety be maintained, that we are endowed with four souls, namely, with a rational, a reminiscent, an active, and a sentient one, as that we have two.

IN brutes of the lowest kind there is evidently a sentient principle; but it seems to be devoid of reason or intelligence: in those, however, of a higher class, we can perceive faint traces of something
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like what we call reason and reflection in man. Why, therefore, may not the human mind, which enjoys all the powers belonging to the souls of the lowest creatures, and has also reason superadded to those powers, be allowed sometimes to act as a sentient, and at other times as a rational being, *i. e.* in different capacities?

BUT, if any should yet contend, that the sentient principle, governing the vital motions, is different from the rational, I shall not further dispute the matter with him: since whatever is advanced, in the present essay, upon the subject of the involuntary motions of animals, will hold equally true, whether the sentient and rational soul be supposed distinct, or otherwise.

HOWEVER, although we conceive it to be the most probable opinion, that the sentient and rational principle in man are one and the same; yet we think it a clear point, that the mind does not, according to Dr Stahl and others, preside over, regulate, and continue the vital motions, or, upon extraordinary occasions, exert its power in redoubling them, from any rational views, or from a consciousness that the welfare of the body demands her care in those particulars: for infants, ideots, and brutes of the lowest kind, (which last are destitute of reason), perform those motions in as perfect a manner as the wisest man; and the mind, when life is endangered by the too violent circulation of the blood, neither does, nor can moderate the motion of the heart. If the contraction of the heart were owing to any reasoning or consciousness of its being necessary to the continuance of health or life, the mind ought to have a power of restraining the uniform motions of its auricles and ventricles, or of repeating them at shorter or longer intervals, notwithstanding their having become, like those of the eyes, in a manner necessary, through long habit: for though we cannot, indeed, move our eyes in every different direction, yet we can restrain or vary their uniform motions as we please.

FURTHER, if there were any exercise of reason necessary to the continuance of the vital motions, the mind ought certainly to be conscious of it; since, in every ratiocination respecting action, there must first be a comparison of things, and then, in consequence of
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that comparifon, a preference or election: but, I believe, few will maintain, that the mind can compare two or more ideas, and thence form certain conclufions and determinations, without being confcious, in any degree, of what it has been all the while employed about: for though, when we are folicitoufly engaged in any action, involved in any thought, or hurried away by any paffion, we may be unconfcious of the impreffions made by material caufes on the organs of fenfe *; yet we cannot but be fenfible of the ideas formed within us by the internal operation of our minds, becaufe their very exiftence depends upon our being confcious of them, and is at an end, as foon as either we attend not to, or forget them: to fay therefore that fuch ideas may be formed and exift in the mind without confcioufnefs, is, in effect, to fay that they may and may not exift at the fame time.

ADD to this, that the motions excited by any pain or irritation, are fo instantaneous, that there can be no time for the exercife of reafon, or a comparifon of ideas in order to their performance; but they feem to follow as a neceffary and immediate confequence of the difagreeable perception. And as the Deity feems to have implanted in our minds a kind of fenfe refpecting morals, whereby we approve of fome actions and difapprove of others, almoft instantly, and without any previous reafoning about their fitnefs or unfitnefs; a faculty of fingular ufe, if not abfolutely neceffary, for fecuring the interefts of virtue among men; fo I fhould think, the analogy will appear eafy and natural, if we fuppofe our minds fo formed and connected with our bodies, as that, in confequence of a *ftimulus* affecting any organ, or of an uneasy perception in it, they fhall immediately excite fuch motions in this or that organ or part of the body, as may be moft proper to remove the irritating caufe; and this

* To avoid all metaphyfical difputes about different degrees of confcioufnefs; I defire it may be underftood, that here and in other parts of this effay, when I fay we are not confcious of certain impreffions made on the mind by the action of material caufes on the organs of the body, I mean no more, than that we have no fuch confcioufnefs or perception of them, as either convinces us of their exiftence when prefent, or enables us, by the help of memory, to recall them when paff.

this without any an previous conviction of such motions being necessary or conducive to this end. Hence, men do not eat, drink, or propagate their kind, from deliberate views of preserving themselves or their species, but merely in consequence of the sensations of hunger, thirst, &c.

THE mind, therefore, in carrying on the vital and other involuntary motions, does not act as a rational, but as a sentient principle; which, without reasoning, is as certainly determined by an ungrateful sensation or *stimulus* affecting the organs, to exert its power, in bringing about these motions, as is a scale which by mechanical laws turns with the greatest weight.

THE general and wise intention of all the involuntary motions, is the removal of every thing that irritates, disturbs, or hurts the body; hence, those greater motions of the heart, in the beginning of fevers, small-pox, measles, &c. when the blood, by the mixture of some particular *miasma*, acts as a stronger *stimulus* than usual upon that organ. Nevertheless, as in many instances the best things may, by excess, become hurtful; so this endeavour to free the body, or any of its parts, from what is noxious, becomes unhappily, sometimes, so violent as to threaten the destruction of the animal fabric. But, in the main, this faculty must be accounted highly beneficial; since, without it, we should constantly have cherished within our bodies the lurking principles of diseases, slowly indeed, and by imperceptible degrees, but not less surely, ruining our health and constitutions.

UPON the whole, there seems to be in man one sentient and intelligent PRINCIPLE, which is equally the source of life, sense, and motion, as of reason; and which exerts more or less of its power and influence, as the different circumstances of the several organs actuated by it may require. That this principle operates upon the body, by the intervention of something in the brain or nerves, is, I think, likewise probable; though, as with regard to its particular nature, I presume not to offer any uncertain conjectures. I shall only observe, that, perhaps, by means of this connecting *medium*, the various impressions made on the several parts of the body,

body, either by external or internal causes, are transmitted, and perceived by the mind; in consequence of which it may determine the nervous influence variously into the different organs, and so become the cause of all the vital and involuntary motions, as well as of the animal and voluntary. It seems to act necessarily, and as a sentient principle only, when its power is exerted in causing the former; but, in producing the latter, it acts freely, and both as a sentient and rational agent.

THE bodies of brute animals are actuated by a principle of a like kind with that which is placed in man, but greatly inferior with regard to the degrees of reason and intelligence which it possesses: in the more perfect brutes, this principle is plainly intelligent as well as sentient; and their actions so evidently shew them to be endued, not only with a good memory, but with reflection and some degrees of reason, that it is surprising to find Descartes and his followers seriously maintaining them to be mere machines formed entirely of matter, and, as it were, so many curious pieces of clock-work wound up and set a-going. Nor is it less surprising that some theological writers, after once admitting all the actions of the most perfect brutes to result from mere mechanism, should not have been aware that the ascribing every action in man to no higher a principle, would be a natural and easy consequence.

IN the inferior orders of brutes the appearances of reason and reflection are more obscure; and, in the lowest species of animals, there are no marks of intelligence, nor do we observe them to differ otherwise from vegetables, than as they are endued with some degree of sensation and self-motion.

Obj. II. It may be alledged, That the vital motions cannot be owing to *stimuli* affecting the mind in the manner above explained, since we are not conscious of any such thing.

Answer. THIS may be owing either to the gentleness of the irritation, or to our having been long accustomed to it, perhaps from the beginning of our lives.

" We all know, that such ideas as but slightly affect us, and soon give place to succeeding ones, are quickly forgot; nay, that

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impressions,

impressions, which are very faint in themselves, or lost amidst stronger ones, are frequently neither attended with consciousness when present, nor remembered when past. In the streets, as many persons of our acquaintance are every minute presented to the mind, as their pictures are painted on the *retina*; yet if we are alone, and have our thoughts strongly turned upon a particular subject, or else are deeply engaged in conversation, we are often not conscious of the presence of those people before our eyes, nor remember that we saw them, after they are gone. If we turn our eyes towards the azure sky at noon, we cannot, by the utmost attention, observe any of the stars; and yet it is certain, that, at that time, there are images of every star in the hemisphere formed upon the bottom of our eyes: for the stellar light must penetrate through the same quantity of sun-beams to reach us in the night as in the day, allowance being only made for the inconsiderable depth of the earth's atmosphere*.

THE sensation arising from the impetuous course of the blood through the pulmonary vessels, is usually so very slight as not to be felt or attended to. But this is far from being the case in asthmatic disorders, or after respiration has been suppressed for some time;

* In this case, we must either suppose, that the impressions, made by the stars on the *retina*, are suffocated and lost in those stronger ones made by the illuminated atmosphere, so as never to reach the *sensorium* in order to excite any idea in the mind, or that if they do reach the sensory, and create correspondent ideas, yet they are so drowned, as it were, in the stronger idea, as to escape our attention and memory. I am not insensible, that there is some difficulty in this matter, and even some appearance of contradiction in the last supposition: for it may well be asked, what is an idea drowned in another, but a perception unperceived? Without pretending to decide, therefore, in this so very subtle a question, I shall only take notice of a fact, which, if duly weighed, would perhaps go as far towards clearing it up as any other consideration whatever. It is well known that Sir Isaac Newton has proved, by a beautiful variety of experiments, that, from the union of simple-coloured rays, are formed compound coloured ones; for example, that a red and yellow ray mingled make an orange; blue and yellow a green one, and so of the rest; and that all the simple-coloured rays combined form a white one. But this discovery is not confined to colours as they exist out of the mind, either in the rays of light, or surfaces of bodies; but is equally true of the ideas of colours in the mind itself: for it appears, by experiments, that the idea of red, and the idea of yellow, confounded in the mind by co-existence or rapid succession, make the idea of orange; the ideas of blue and yellow, that of green, &c. and those of the seven simple colours that of white.

time; for then it is very perceptible, being accompanied with great uneasiness and anxiety. The action of the air, aliments, and bile, upon the intestines, which is the cause of their peristaltic motion, is commonly unperceived by us; but let the *stimulus* acting on the intestines be increased, as is the case when any strong purgative is taken, or when any acrid humours are lodged in the *primæ viæ*, and it will be felt very sensibly. The *stimulus* of light upon the *retina*, which makes the pupil contract, is seldom perceived or regarded, unless when the degree of light is much stronger than what the eye, immediately before, had been exposed to. The action of the returning blood upon the heart, though it be usually imperceptible, in some cases is plainly to be felt: for people, especially such as have weak nerves, after a sudden fright, (which makes the blood return more hastily, and in greater quantity, than usual to the heart), are sensible of a particular feeling from this organ's being more than ordinarily affected by a surcharge of that fluid. In various parts of the body, pulsations, or small alternate convulsions, are sometimes perceived, which, as they keep not time with the beating of the heart, cannot be arterial vibrations, but must be the alternate contractions of muscles, or, rather, of a small parcel of their fibres. There is no sensation of a *stimulus* in the part before these motions begin, or while they continue; and yet, as they frequently happen to people in health, whose brain and nervous system are sound, it is probable that they are owing to some obstructing matter, which over-stretches the fibres of the subtiler vessels, or to acrid particles in the fluids touching the tender nerves of the convulsed part.

^B THE *stimulus* occasioning the vital motions is unperceived by us, not only on account of its gentleness, but also because we have been accustomed to it from our birth. The force of custom is great and unaccountable; what we have been long used to, we become scarcely sensible of, while things which are new, though more trifling, and of weaker impression, affect us remarkably. Thus he who is accustomed to the country, is much affected with the noise and bustle of a populous city: but by use, he daily becomes less and less

fenfible of it, till, at length, he minds it little more than they who have been used to it all their life-time.

THE same seems to be the case with regard to what passes within our bodies. Few persons in health feel the beating of their heart, though it strikes against the ribs, and that too with a considerable force, every second or oftener; whereas the motion of a fly upon one's face or hands occasions a fenfible and an uneasy titillation. The pulsation of the *aorta* itself is unfelt; whilst the unusual beating of a small artery in one of the fingers, from an obstruction of its vessels, becomes very fenfible. Although the blood rushes into the ventricles of the heart with a considerable velocity, and is thence expelled into the arteries with still a greater force; yet we are not conscious of one drop passing that way; otherwise the circulation of the blood could not have remained so long a secret. And if we are not fenfible of the *stimulus* of the air, or of the aliment or bile upon the intestines, (which, however, is allowed to be the cause of their vermicular motion); nor are immediately conscious of the action of *opium* upon the nerves of the stomach, (which yet produces surprising effects over the whole body); why should it be thought strange that we do not feel the *stimulus* of the blood upon the internal surface of the ventricles of the heart, which is more gentle than the latter, and which, as well as the former, custom, that second nature, from the beginning of our lives, has rendered quite familiar and unheeded?

UPON the whole, I hope that I have made it appear, that there is no good reason for denying the vital motions to proceed from *stimuli* affecting the mind, because we are not conscious of it; or for imagining that the blood does not gently irritate the heart, because we do not feel a particular sensation in that organ immediately preceding its several contractions.

Obj. III. IT may be said, That although we are insensible of the *stimuli* affecting the organs of vital motion, either from their slightness or from custom; yet we ought to be conscious of the exertion of the power of the mind in producing those motions.

Answer.

Answer. * THAT a man may, in general, be called conscious of any action, it is not only necessary that he should perceive it, during the time in which it is performed, but also that he should be able to recollect it after it is past: for though one be sensible, while a visible object is before him, that he sees it; yet if he retains not the least memory of it after it is removed out of his sight, he can neither satisfy himself nor others that ever he saw it. In like manner, we cannot be called conscious of an action or volition that is not adverted to when performed, or, as soon as it is over, is entirely forgotten: for as there are some sensations, either so slight in themselves, or so much weakened by the diversion of our attention, that they leave no traces in the memory; so there may be actions and volitions that are either so faint, so habitual, or so much lessened amid stronger and more important exertions of the mind, that they may not only be entirely forgotten, but not so much as taken notice of or reflected upon.

§ BUT setting aside all metaphysical arguments, we may find arguments *a posteriori* sufficient to prove that the mind does perform actions unattended with any consciousness. Thus, altho' we are not conscious of any effort of the mind in producing those motions of the body which tickling the sides or the soles of the feet excites; yet it appears, that, in fact, they proceed from the mind, from the like motions being produced, though in a less degree, by the fear, only, or apprehension of being tickled. Dust, as well as flies and several other insects passing before our eyes, make us shut the *palpebræ*; and yet these motions, which certainly proceed from the mind, are not often attended to, and seldom remembered by us. The contraction of the pupil from light, and of the muscles of the internal ear from sound, has been shewn to arise from an exertion of the power of the mind, of which, however, we are in no degree sensible. As the erection of the *penis* often proceeds from lascivious thoughts, it must be ascribed, in these cases at least, to the mind, notwithstanding our being as unconscious of its influence exerted there, as in producing the contraction of the heart. The sight, or even the remembrance of grateful food, is accompanied with a sudden and copious secretion of *saliva*
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in the mouth of a hungry person: certain ideas excited in the mind are the occasion of a flow of tears from the lachrymal vessels: and the breast of a nurse begins to give milk when a child is brought only near it. The extraordinary motions of the vessels of those parts cannot in any other way be accounted for, than by ascribing them to the mind; of whose action, however, we are no ways conscious.

FURTHER, since, in consequence of certain ideas being excited in the mind, the stomach is immediately affected with a *nausea* and vomiting, it cannot be denied that this is owing to an increased action of the nervous influence, by means of the mind, upon the muscular fibres of this organ; yet we are not more sensible of an exertion of the mind in this case, than we are when vomiting is excited by a dose of *ipécacuanha* or emetic tartar. The want of consciousness, therefore, can be no good argument against the motion of the stomach, whether natural or perverted, being produced by the active power or energy of the sentient principle, which is variously affected by the different *stimuli* applied to the delicate nerves of that organ; and if the idea, only, of a disagreeable sensation in the stomach can occasion, through the influence of the mind, the motions of vomiting, why should not the real sensation in it more remarkably affect the mind, and so excite it to produce the same motions?

WHAT has been said with regard to the motions of the stomach, may readily be applied to those of the heart: for no sooner are certain ideas presented to the mind, than the motion of the heart is increased and accelerated; which must, therefore, I should think, be the effect of an extraordinary action of the nervous power on its fibres consequent upon the emotion raised in the soul: yet of this effort of the mind we are not in any degree conscious. If, therefore, the mind can thus influence the motion of the heart, whilst we are not sensible of its power being directed to that end, it seems not unreasonable to suppose, that the *stimulus* of the returning blood may excite the sentient principle to bring the heart into contraction, although we are not conscious of any such exertion of its power.

BUT

BUT the objection against the mind's being concerned in the vital and other involuntary motions, drawn from our not being conscious of its interposing for that purpose, is removed, by considering that many even of the voluntary motions are performed, when we are insensible of the power of the will exerted in their production. Thus, while in walking, we either meditate by ourselves, or converse with others, we move the muscles of our legs and thighs, without attending to it, or knowing what we are doing. We are not sensible of the eye-lids being kept open by the continued operation of the will; but yet, when drowsiness and sleep steal upon us, we find it requires a considerable effort to prevent the falling down of the superior *palpebræ*. The same thing is true of the action of the muscles which support the head. The most probable account of our ignorance of these things seems to be this, *viz.* that we not only acquire, through habit, a faculty of performing certain motions with greater ease than at first, but also, in proportion as this facility is increased, we become less sensible of any share or concern the mind has in them. Thus a young player upon the harpsichord, or a dancer, is, at first, solicitous about every motion of his fingers, or every step he makes, while the proficient or masters in these arts perform the same motions, not only more dexterously, but almost without any reflection or attention to what they are about.

SOME indeed have gone so far, as to deny that even the voluntary motions are owing to the mind as their proper cause, and have thought the direction of the voluntary muscles, in order to perform the various motions of the body, to be an office which its faculties are not equal to. But if these motions be not owing to the mind, from what cause, external or internal, do they proceed? They cannot be owing to the powers of the body alone; and it is in vain to attribute them to any law which it may be pretended the Deity has established; since a law can produce no effect of itself, and, without some agent to execute, it is only a mere name or empty sound: they must, therefore, be ascribed either to the immediate agency of the SUPREME BEING, or to that of some general inferior NATURE which HE has constituted for this purpose, or to the
energy

energy of a particular active principle united with the body. The first two suppositions are indeed possible, but not probable, as is the last; whence it may be inferred, that not only the voluntary motions of which we are immediately conscious, but those also which we do not advert to, proceed from that sentient and intelligent principle with which the Creator has animated our bodies; whose powers and operations, it must be owned, are, in many instances, as much above our knowledge as is the nature of its union with the body, or the manner of their reciprocal action upon each other.

Obj. IV. IF the vital motions were owing to the mind, they should be under its dominion or controul; and we ought at any time to be able to suspend or vary these motions at pleasure.

Answer. " IN all actions which are the result of reasoning and deliberation, man evidently appears to be a free agent: for he has it in his power, after weighing all motives and circumstances, to prefer this or the other action, or to abstain from acting altogether. But there are actions, towards the performing of which we are not determined by reason, and where the mind is not a free but a necessary agent. Of this kind are the involuntary motions of those muscles whose fibres are affected by any considerable *stimuli*; for the application of external objects to their proper organs, does not more certainly or immediately excite corresponding ideas in the mind, than certain uneasy sensations produce motions of the body. As we cannot, therefore, hinder ourselves from seeing every object which is painted on the bottom of the eye, nor from hearing every sound which affects the ear; so neither can the mind refrain from exerting its power of moving a muscle whose sensible fibres are strongly affected by a *stimulus*. And as no body denies that it is the mind which sees colours and hears sounds, (because, whenever the external causes exciting these are applied to their proper organs, we can, by no effort of the will, prevent ourselves from seeing or hearing, nor can see and hear objects or sounds different from what these impressions naturally represent); so it must be unreasonable
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to say that the vital and other involuntary motions cannot arise from the energy of the mind, because the will has no immediate power over them.

AN action is denominated free, from the agent's having willed or chosen it, when he had a physical power of doing otherwise; thus the action of swallowing poison is said to be free, when a person chooses it, and might have refrained from it; but the convulsive motions of the stomach and diaphragm which soon ensue upon taking it, are strictly involuntary; since the mind cannot, by any effort or exertion of its power, prevent them; being as certainly determined to move these parts violently, from the disagreeable sensation which the poison excites, as a stone is in falling to the ground, or a scale in inclining to the side where there is the greatest weight, from the principle of gravity. The only difference in these cases is, that in the first, the cause operates by means of a living sentient principle; and in the last, acts upon inert and lifeless matter.

As the actions which follow an irritation of our muscles, or any uneasy sensation in the body, are not performed by the mind, in consequence of any reasoning, or from any view of their being immediately necessary, or conducive to the welfare of the body; so neither do they flow from custom or habit; since new-born children perform them as well as the oldest and most experienced man. Infants, as soon as they come into the world, perform the action of breathing, though quite unaccustomed to it before; they shut their eye-lids upon the approach of light, vomit when their stomach is oppressed, suck when hungry, sneeze, or cough, upon any irritation of the membrane of the nose or windpipe, and void their excrements and urine, when these excite an uneasy sensation in their intestines or bladder.

It has been said, that in the "beginning of life the vital motions
"were altogether voluntary; and, that it is only from use or custom,
"that they have become so necessary, as to make it impossible for
"us to hinder their going on in their usual manner*." But al-

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though

* Dr Porterfield's Treatise on the Eye, vol. 2.

though custom may enable us to perform some actions with surprising facility and little or no attention, yet it will not render the motions of muscles absolutely involuntary, which were originally voluntary. There is no instance in the human body of any muscle, whose motion can be fairly proved to have been voluntary in the beginning of life, that has by custom or habit become so far independent on the will, as to be in no degree under its immediate controul. Thus, although we cannot put in motion the muscles of one of the eyes, without, at the same time, moving the muscles of the other eye, in the way which we have always been accustomed to; yet we can move these muscles as slowly, or swiftly, as we please, or restrain their motions altogether: and the learned M. de Haller has well observed, against the followers of Stahl, that the muscles of the eye-lids, and those which serve to erect the back, though they are almost constantly employed, except in time of sleep, nevertheless continue to be subject to the will *. Further, since the motion of every voluntary muscle in the body becomes, at once, involuntary, when its fibres or nerves are irritated, there seems to be no need of having recourse to custom or use, in order to account for our having no power over the motions of the heart and alimentary canal.

It remains, therefore, that the motions performed by us in consequence of irritation, are owing to the original constitution of our frame, whence the soul or sentient principle, immediately, and without any previous ratiocination, endeavours by all means and in the most effectual manner, to avoid or get rid of every disagreeable sensation conveyed to it by whatever hurts or annoys the body.

WHEN the organ is not extremely sensible, or when the *stimulus* is very slight, or is applied to some distant part, and not immediately to that which is to be moved, we can, by an effort of the will, restrain those motions, which otherwise would follow; but if the nerves of the part be more delicate, and susceptible of the smallest impression; if the *stimulus* be stronger and applied immediately to the organ which is to be brought into action; then the motions

* Not. in Boerhaave Institut. med. vol. iv. p. 588.

tions which follow are necessary, and cannot be controlled by the power of the will; because the mind is more strongly affected by the uneasy feeling excited by the irritation, than by any arguments or motives it can possibly present to itself: for, although we may, in some cases, be fully convinced that these motions tend to the destruction of our body, yet even this consideration does not enable us, either to lessen or put a stop to them*. To illustrate this matter by a few examples. In voiding the excrement and urine, the contraction of the abdominal muscles and diaphragm is usually in some measure voluntary, and can be restrained at pleasure, because the *stimulus* is not only slight, but applied to a distant part; but in a violent *tenesmus* or stranguary, the motion of these muscles becomes necessary and convulsive, not because the mind is less concerned in this last case than before, but because the painful sensation compels it to act. If the internal membrane of the wind-pipe be slightly irritated, we can restrain coughing; but if the tickling be strong, we lose that power. When the *tunica cornea* or *conjunctiva* of the eye is gently stimulated, we can, by an effort of the will, prevent the shutting of the *palpebrae*; but when any thing very acrid is applied to these parts, the eye-lids are moved necessarily. Although the contraction of the pupil arises from the action of light upon the *retina*, and not upon the fibres of the *iris*; yet this motion is altogether involuntary, on account of the extreme sensibility of the irritated part.

THE motions of the heart and alimentary canal are wholly necessary, because the nerves of these organs are endued with a peculiar

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* A subtle defender of the Stahlian doctrine has at last been obliged to acknowledge, that the vital and other involuntary motions of animals are morally necessary, although not truly or physically so (a.) But those actions which are only morally necessary, are so far voluntary as to entitle the agent either to praise or blame; and they are attended with a consciousness of liberty: while, on the contrary, the motions of the heart and alimentary canal, as well as of such voluntary muscles as are strongly irritated, are no indications of the moral dispositions of the mind, nor are they attended with the least consciousness of their being free, or that we could have restrained them by the power of the will. As these motions, therefore, are most certainly not voluntary; so neither can they, with any propriety, be said to be only morally necessary.

(a) Dr. Porterfield's *Treatise on the Eye*, vol. ii. p. 154.

cular sensibility, and the *stimulus* is immediately applied to them *. Nor can the mind lessen the violent contractions of the heart in a fever, however conscious it may be of the danger arising from too impetuous a motion of the blood; because the heart being more strongly stimulated than usual by that fluid, the sentient principle, in order to expel the irritating cause, is excited to contract this muscle with proportionably greater force. The motions of the inspiratory muscles can be accelerated, retarded, or altogether stopt, as often as we please; because the *stimulus* exciting them is not immediately applied to their fibres or nerves, but to the blood-vessels of the lungs, which are not very sensible, and with which they have little or no connection. However, as often as this *stimulus* is much increased, by the difficult passage of the blood through the lungs, and there is an immediate danger of suffocation, the motion of those muscles becomes more necessary, and almost ceases to be under the power of the will. In a fever, when, from an obstruction or perverted motion of the fluids in the brain, or its membranes, the patient talks of seeing and hearing things which are neither present nor spoken, he may be readily convinced of his error, provided the *delirium* be slight, if otherwise, we endeavour in vain to correct his wrong judgment by reason or argument, since the disordered state of the brain makes a stronger impression upon the mind, than any arguments; yet acrid cataplasms applied to the soles of the feet, as they give great pain, and so make a strong impression on the *sensorium commune*, will often lessen, and sometimes entirely remove such a *delirium*.

BUT

* Some physicians of note have concluded the heart to be possessed only of a small degree of sensibility, because brute animals, after their *thorax* is opened, shew no signs of new pain when their heart is wounded. But, unless the heart were endued with a more acute feeling than the skin and other parts that are hurt in opening the chest, the sufferings of those animals ought not to be sensibly increased by pricking or cutting their heart. Add to this, that after the *thorax* is opened, and the heart laid bare, the animals being in a dying state, have all their feelings much impaired.

Since the sensibility of the medullary part of the brain has been inferred from the convulsive motions occasioned by irritating it, may we not conclude, that the heart, whose mo-

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^a BUT the objection against the mind's producing the vital motions, drawn from their being involuntary, must be weakened by observing that there are a variety of motions equally independent upon our will, which yet are owing to the mind. Thus, as has been already observed, the contraction of the pupil from light, and the

tion is remarkably accelerated even by very gentle *stimuli*, is endued with no inferior degree of feeling? and this seems to be confirmed, by observing, that, the motion of the heart is often greatly quickened by a degree of acrimony in the blood imperceptible to our senses, and by the chyle, which is much less acrid than those aliments from which it is prepared, and which the stomach and intestines bear without any pain or remarkable increase of their peristaltic motion. I shall only add, that the internal surface of the heart, to which the returning venous blood is applied, appears from experiments (*a*) to be much more sensible than its exterior part, which is covered with a continuation of the *pleura*; therefore that fluid, which is a *stimulus* peculiarly adapted to the nerves of the heart (*b*) must affect it more strongly than sharper liquors applied to it outwardly.

The objection to the sensibility of the heart, drawn from its having been found much diseased after death, although the patients had complained of no acute pain within the *thorax*, seems to be of little force; for I have often met with inflammations, suppurations, and mortifications in several of the abdominal *viscera*, nay, even in the intestines themselves, in the bodies of patients who had, during their life, complained rather of an uneasiness than an acute pain in their belly: and practical physicians well know, that according to the degree of the disease, and peculiar constitution of the patient, inflammatory as well as other disorders are attended with very different degrees of pain. Nor is the instance quoted from Harvey, of a young nobleman who felt no pain when his heart was touched, of much weight; for, in this case, the heart was defended by a thick insensible fungous or callous covering (*c*); whence handling it gently, neither occasioned any uneasy sensation nor disturbed its motion. And, indeed, as neither the membrane which lines the nose and *fauces*, nor the tongue, *glans penis*, and intestines (*d*), although among the most sensible parts of the body, are affected with any painful sensation when they are gently touched; it is probable, that were the *pericardium*, or even the surface of the heart itself, in a sound state, to be touched with one's finger, no pain would be felt, but a different kind of sensation, which might perhaps disturb its regular motion, and occasion some degree of faintness.

The *stimuli*, which, in a natural state, excite the alternate motions of the heart and alimentary canal, do not affect those organs with any painful sensation, but with a slight titillation, which in some cases is as effectual to produce motion, or even more so than pain: Thus when the neck, the sides or the soles of the feet are tickled, violent motions of the muscles of the head and trunk of the body or of the limbs ensue; which however do not happen, when those parts are affected with an acute pain, by being pricked or cut with a sharp instrument.

(a) Aët Gotting vol. 1.

(b) See above, p. 21. 22. & 56. &c.

(c) Harvei de generat. animal. exercitat. 52.

(d) See Physiological essays edit. ii. p. 156.

the motions of the body from tickling, or the apprehension of it, undoubtedly flow from the mind, notwithstanding their being involuntary. The shutting of the eye-lids, when a blow is aimed at the eye, is another instance of a motion performed by the mind in spite of the will; for as the threatened blow does not, by any corporeal contact, affect the orbicular muscle of the *palpebræ*, its contraction must therefore be deduced from the mind's being moved to perform this action from the apprehension of something being ready to hurt the eye: and if there are some who, by an effort of the will, can restrain this motion of their eye-lids, yet this does not proceed so much from the mind's making no attempt, in consequence of the apprehended danger, to close the *palpebræ*, as from the superior eye-lid's being kept up by a strong voluntary contraction of its *levator* muscle. We cannot, by an effort of the will, either command or restrain the erection of the *penis*; and yet it must be owing to the mind; for sudden fear, or any thing which fixes our attention strongly and all at once, makes it quickly cease. The titillation, therefore, of the *vesiculæ seminales* by the *semen*, lascivious thoughts, and other causes, only produce the erection, as they necessarily excite the mind to determine the blood in greater quantity into the cells of the *penis* *. A shocking sight, or a disagreeable sound, will often, in an instant, excite a *tremor* or shivering over the whole body; which cannot be owing merely to the mechanical action of light upon the eye, or of sound upon the ear; since, when the external organs are unaffected by such things, their idea, recalled by the mind, can of itself produce a similar effect: this motion, therefore, though it be involuntary, and can neither be performed nor stopt at pleasure, must be owing to the mind or sentient principle.

IF, therefore, we have found various involuntary motions arising from the mind, it can be no proof against the vital motions flowing from the agency of the same principle, that they are involuntary: and if the motions of the voluntary muscles themselves become involuntary, as often as they are excited into action by *stimuli* applied

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* Vid. Sect. vi. No. 4. above.

to their fibres, or nerves, it can be no wonder that the motions of the heart and alimentary canal are necessary, and independent on the will, since these organs are perpetually exposed to the alternate action of a *stimulus*.

If it be asked, why, when neither light nor sound affect the muscles of the *uvea* and internal ear, we cannot at pleasure move them; or why we cannot at any time, by an effort of the will, immediately produce an erection of the *penis*? it may be answered, That as men, by having their ears tied down when young, lose the power of moving them, though there seem to be muscles destined by nature for this purpose; so the mind, through disuse, may have lost its power of moving the above-mentioned muscles at pleasure, even when they are not acted upon by a *stimulus*: or why may we not, for wise purposes, be so framed by nature, that the mind, while it can at pleasure contract the greatest part of our muscles, may have no power over others, whose motions are to be regulated only by certain sensations; since these will never fail to excite the sentient principle into action, when it is necessary or proper? Whereas, if those motions were subject to the will, it is probable, that men, by a perverse use of it, would in many cases destroy their health, and endanger their lives. And I imagine, that the mind's want of power over the motion of the heart, is not only owing to its being continually acted upon by a *stimulus* *, but in part to an original constitution †; and that though we should suppose that organ for a little while free from every degree of irritation, yet the mind, by an effort of the will, could not move it. Thus, notwithstanding the mind remains present with the body, and ready to actuate it in a *syncope*; yet it can neither directly renew the motion of the heart after it has ceased, nor communicate a stronger contraction to it when it is going to fail: and there is no reason to think, that those animals which lie in a death-

* Sect. i. No. 12. and 13. above.

† By an original constitution, I here mean no more, than that we are so formed, that the mind, which can at pleasure move most of the muscles of the body, has, from the beginning of life, no power to move the heart and other involuntary muscles, unless when it is excited by *stimuli* acting upon them, or upon some neighbouring part with which they have a peculiar sympathy.

death-like state during the winter-season, have, when they begin to revive in the spring, any more power over the motion of their heart, than those in whom its motions continue without any such interruption from the beginning to the end of their lives.

WE need not, therefore, with an ingenious author, have recourse to any crossings or *entrelacements* of the nerves of the vital organs, or to their proceeding from different parts of the brain, in order to account for their not being subject to the power of the will *; especially since we see that the motions of the *uvea* and muscles of the internal ear, notwithstanding their nerves are without the above conditions, are equally involuntary with those of the heart; that the muscles of the arm, whose nerves have these crossings, are, nevertheless, moved by us at pleasure; and that, in short, even the voluntary muscles, when affected by any remarkable *stimulus*, cease to be under the controul of the will †.

Obj. V. THE mind can only perceive distinctly one idea at once; and therefore must be incapable to attend to and govern all the vital and involuntary motions, which are so numerous ‡.

Answer. THIS objection is chiefly levelled against the opinion which supposes the vital motions to be regulated and carried on by the mind as a rational agent, and therefore does not much affect our theory: for whether the mind can distinctly apprehend more ideas than one at a time, or no, yet surely it can and does feel various sensations in different parts of the body at one and the same time; and we know that it can move many of the voluntary muscles in the same instant. Why, therefore, may it not, in consequence of the perception of various *stimuli* affecting the different vital organs, move them alternately?

BUT further, when Mahomet Caratta, the famous equilibrist, stood with one foot on the slack wire, tossing, with his hands, six or seven balls up into the air, and catching them again, was he not
attentive

* *Essais anatomiq.* p. 702. & *Element. physiolog.* p. 72.

† See above, sect. i. No. 12. and 13.

‡ Haller. not. in Boerhaave *institut. med.* p. 589.

attentive to more than one thing at once? In this case, the *equilibrium* of the body was to be preserved, the balls were to be taken out of his girdle, they were severally to be thrown up into the air, to be caught as they came down, and tossed up again; and these motions, which followed each other with surprising quickness, were continued for some considerable time. Any man can hear a sound and perceive a particular colour at the same time; and though ever so attentive to these, he will not fail, if a fly happens to run along his face, to drive it off with his hand, that he may avoid the tickling sensation which it excites. In like manner, how much soever the mind may be busied with its own thoughts, or the ideas of external objects; yet it is ever ready to perceive and feel the various *stimuli* which alternately affect the vital organs, and, in consequence of this, to continue their motions. Nor is there any need of an infinite wisdom in the mind, as some have objected, to enable it to carry on the several vital and other involuntary motions, with different and always varying degrees of force and quickness, according to the different circumstances of the body; since, in so doing, the mind has no particular wise ends in view; nor is it influenced by any rational motives, but merely by the *stimuli* affecting the several organs, *i. e.* it acts as a sentient, and not as a rational principle. In the Author of nature, however, who has framed both the soul and body, and thus adapted them to each other, we ought, as upon many other accounts, so also upon this, to acknowledge a wisdom infinite and unsearchable!

IN contemplating the various motions of animals, we observe a striking analogy; a remarkable agreement in some things, and a disagreement in others.

I. SOME of the voluntary motions, by the force of custom and habit, come at length to be performed with little or no attention of mind; and, though we have full power to begin or stop them when we please, yet they become so far independent of the will, that we

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can only perform them in a certain way. Of this the uniform motions of the eyes are an example.

2. NEARLY a-kin to these are the mixed motions, or those of a middle nature between the voluntary and involuntary; such as respiration, and the motions of the eye-lids when any thing slightly irritates the *cornea*. These agree with the motions from habit, in being often performed without consciousness; but in this they differ, *viz.* the former proceed from a *stimulus*, and become altogether involuntary when that is increased; while the latter owe their beginning to an effort of the will, and are always subject to its controul.

3. THE involuntary and mixed motions, agree, in general, in proceeding from a *stimulus*, and in being mostly performed without consciousness; but differ, in the latter's being partly, and the former not at all under the power of the will. Some of each of these motions never cease, but go on alternately through life; while others are only excited on certain occasions: among the former kind are the motions of the heart, lungs, and alimentary canal: of the latter are the contraction of the pupil, eye-lids, and muscles of the internal ear.

4. IN some of the involuntary motions, we are neither conscious of the *stimulus*, nor of the effort of the mind in consequence of it; as in the motion of the heart, and the ordinary vermicular contraction of the stomach and intestines. In others, we are sensible of the irritation, or disagreeable perception exciting them, but not of any exertion of the power of the mind: such are the convulsive contractions of the stomach, diaphragm, and abdominal muscles in vomiting, of the diaphragm in the hiccup, of the intestines in purging, and of the *acceleratores urinæ* in expelling the *semen*.

5. WITH respect to the mixed motions: in those of the eye-lids, so far as they are of this kind, we are sensible of the irritation, or cause exciting them, though rarely so of any effort of the will. In
that

that of respiration, neither the *stimulus* affecting the lungs, nor the effort of the mind in consequence of this, are usually perceived; yet, as often as we please, we can suspend or vary this motion, as freely as those of the eye-lids. In the motions of the diaphragm and abdominal muscles, in expelling the excrement and urine, which are also of the mixed kind, we are perfectly sensible of the *stimulus*, and frequently of an exertion of the power of the mind in consequence of it; yet, when the irritation is very great, these motions become wholly convulsive and involuntary.

6. FURTHER, it appears, that as in all the works of nature, there is a beautiful gradation, and a kind of link, as it were, betwixt each species of animals, the lowest of the immediately superior class differing little from the highest in the next succeeding order; so in the motions of animals, something similar may be observed; the mixed motions, as they are called, and those from habit, being the link between the voluntary and involuntary motions.

Lastly, FROM what has been advanced in this and the preceeding sections, with a design to shew what share the mind has in producing the vital and other involuntary motions, it follows, that the human body ought not to be regarded (as it has too long been by many physiologists) as a mechanical machine, so exquisitely formed, as, by the mere force of its construction, to be able to perform, and continue the several vital motions *; actions far above the powers of mechanism! But as a system, framed indeed with the greatest art and contrivance; a system! in which the peculiar structure of each part is not more to be admired than the wise and beautiful arrangement of the whole; yet a system whose functions are all owing to the power and agency of an immaterial sentient principle to which it is united, and by which every part of it is animated and put in motion.

IN accounting for the vital and other involuntary motions of animals, we have shewn, that they are all owing to irritation; and

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have

* See Heister's Dissert. de præstant. medicin. mechanic. p. 22. 25. 51. 69.

have pointed out the particular *stimuli* applied to the several organs, and exciting them into action; we have further shewn, that these *stimuli* can only produce their effects by the influence which they have upon the mind or sentient principle. But how does the mind put the muscles into motion; what is the *material* cause in the brain, nerves, and muscular fibres, which it employs as its instrument for this purpose; what is the intimate structure of a muscular fibre, or the precise manner in which the nervous influence acts upon it, when it produces its contraction? These are questions which we have wholly avoided, being persuaded, that whatever has been hitherto said on these subjects, is merely speculative; and that to offer any new conjectures on matters so much involved in darkness, and where we have neither experiments nor natural appearances to support us, is to load with a new burden a science already labouring under too many *hypotheses*.

To finish our account of the spontaneous motions, it now only remains, that we inquire into the reasons why the vital motions continue in time of sleep, and why the muscles, or a few of their fibres, are often observed to move for some time after death or their separation from the body.

S E C T. XII.

Of the reason why the vital motions continue in time of sleep.

THE reason why the vital organs are continually agitated with alternate contractions while the other muscles of involuntary motion are contracted on certain occasions only, will appear from what has been already offered; for we have seen that the former are always exposed to the action of *stimuli*, the latter only at particular times. But since, during sleep, the organs of sense become less fit for receiving the impression of external objects, and the voluntary muscles are, in some measure, relaxed and remain inactive,

active, it may be asked, why the vital motions do not at this time either cease altogether, or become sensibly more languid?

To say here, that the vital motions must therefore go on without being weakened or disturbed, because their organs are equally acted upon by their proper *stimuli*, both when we are awake and sleeping, would be an incompetent answer to this question; for though the stimulating cause be granted to continue the same, yet if the aptitude in the organ for motion be lessened, the effect must be the same, as though the *stimulus* were weakened or intirely wanting. The difficulty, therefore, which we are to endeavour to remove, is, why the vital organs should not, like the organs of sense and muscles of voluntary motion, be so far affected by sleep, as to become less fit or able to perform their usual functions?

SLEEP has been supposed to be owing to some change produced in that part of the body which anatomists, distinguishing it from the *cerebellum*, call the BRAIN. And this opinion seems to have been rendered probable by some instances of people who, having lost part of their skull, were immediately seized with sleep, whenever their brain was gently pressed; and by those experiments which shew, that, instead of sleep, death itself, or at least a *syncope*, is the effect of a like compression upon the *cerebellum*. If, therefore, it could be made appear, that the vital organs have their nerves chiefly from the *cerebellum*, and not from the brain, it might be presumed, that their motions continue in time of sleep, because the nerves of the *cerebellum* are not then affected like those of the brain.

Now there are many experiments of Vieussens *, Ridley †, and others, shewing, that respiration and the motion of the heart are quickly stopt upon wounding the *cerebellum*; but that wounds in the brain produce little or no change in those motions. On the other hand, several authors of reputation and unquestioned veracity assure us, that, in their experiments, the vital motions continued for some considerable time after the *cerebellum* had been cut in pieces. However, it does not appear from other experiments of these very
authors,

* Neurograph. lib. 1. cap. 20.

† Anatomy of the brain, chap. 17.

authors, that wounding the brain or cutting it in pieces affected the vital motions more, or even so much, as the same treatment of the *cerebellum*. Are we then to conclude, from these experiments, that neither the brain nor *cerebellum* are necessary to the motions of the vital organs? By the like kind of reasoning, it would seem, that the nerves also, and the influence they may have, are unnecessary in the producing of these motions, since that of the heart has been known to remain a considerable time after the intercostals and eight pair of nerves have been cut. Do these experiments, therefore, which prove too much, prove nothing at all? Far otherwise. The true inference seems rather to be this: That, since various experiments concur in shewing the *cerebellum* to be more concerned in the vital motions than the *cerebrum*, while none at all can be produced in proof of the *cerebrum* being more immediately necessary to these than the *cerebellum* *, it follows, that the vital organs have their nerves, either wholly or principally, from the latter.

BUT though the *cerebellum* be the chief source of the vital nerves, yet its destruction does not put an immediate stop to the vital motions, for the same reason that cutting off the head, or tying the intercostal and eight pair of nerves, does not produce this effect; because the branches from the spinal marrow which join the intercostals, together with the spirits (if I may be allowed to call the influence of the brain by that name) remaining in the trunks of the nerves and fibres of the heart, are sufficient to keep up these motions for some time: in man, perhaps, only for a few pulsations, in young dogs or cats for several hours, and in a tortoise for several months; which last animal, not to mention other differences, has its spinal marrow remarkably large: nay, the motion of the hearts

* In the accurate Dr Kaau's experiments, the vital motions continued in a dog, above eight hours after the medullary part of the brain was reduced to a pulp; but when the medullary substance of the *cerebellum* was treated in the same way, though they did not cease instantly, yet they began to fail in a few minutes (a). When the *cerebellum* was wounded, without touching the *cerebrum*, the motion of the heart failed sooner than when the brain alone was wounded (b).

(a) Impet. faciens, N. 325.

(b) Impet. faciens, N. 326.

hearts of many animals, after they are taken out of their bodies, affords us ocular demonstration, that the nervous influence, lodged in the fibres of the heart and in the smaller filaments of the nerves, is sufficient to continue the motions of this muscle for some time, or to enable it to perform a great number of contractions *.

THE instances given us of animals, whose *cerebella*, upon opening them, were found scirrhus, corrupted, or otherwise diseased, avail no more, towards proving that the vital organs don't derive their nerves chiefly from this part, than do the histories of ossified and petrified brains, or of monsters born with no brain at all, towards making it a clear point that the brain and nerves are, in fact, not the source of sense and motion †.

BUT as it is now acknowledged, by the best anatomists, that the 5th pair of nerves proceeds from the *cerebellum*, which, however, is not distributed to the vital organs, but chiefly to the teguments of the head, and muscles of the face, whose motions are voluntary; it may be said, that allowing the nerves of the vital organs to come from the *cerebellum*, yet, why should not they suffer the same diminution of their vigour in time of sleep, as do those parts to which the fifth pair of nerves is distributed?

IN order to throw some light on this point, it may be of use to inquire, briefly, into the effects of sleep on the sentient and moving power of the nerves; for from this it will appear, whether or not the vital organs may reasonably be expected to continue their motions during sleep, even supposing them to be equally affected by it, as are the muscles of voluntary motion.

IN time of sleep, the *sensorium commune* remains, in a great measure, at rest, and consequently the usual exercise of the internal senses, and the voluntary motions, are suspended; but the nerves distributed to the several parts of the body do not lose either their
sentient

* A learned author indeed, has concluded, from the continuance of the motion of the heart after its separation from the body, that it cannot be owing to the influence of the nerves; but this circumstance does not seem to warrant such a conclusion; and that the moving power of all our muscles, the heart not excepted, proceeds from the nerves, I have endeavoured to prove in my *Physiological Essays*, edit. 2. appendix, pag. 245: &c.

† See Sect. 1. No. 1. above.

sentient or moving power. It must be acknowledged, however, that in time of sleep our feelings are less acute than when we are awake; and hence the same irritation in the *trachea* makes us cough less in the former than in the latter state; but, notwithstanding this, the nerves continue to be so sensible, that not only pricking any part of the body with a pin, but touching it gently with one's hand, or whispering softly into the ear, will break off sleep in many people. And as the feeling of the parts of the body, although it be impaired, is not suspended in time of sleep, so neither is their motion. Thus, altho' upon one's falling asleep, the muscles which support the head cease to act with their usual force, because the will does not exert its power in contracting them in the same manner as when we are awake, yet neither these muscles nor those of the face are at that time deprived of the nervous power; for, if a hair only be drawn along the face of a child when it is asleep, the muscles of the cheeks and mouth will be variously contracted; and if this irritation be continued, the child will at length move its head, and perhaps turn its whole body. In like manner, uneasy feelings in time of sleep often make us move and toss about in bed; and some people not only talk when asleep, but get up and walk from one room to another.

It appears, therefore, that in time of sleep there is no such diminution of the sentient or moving power of the nerves as to unfit the muscles of voluntary motion for action, provided the mind be excited, either by external *stimuli*, or by certain ideas produced in the *sensorium commune*, to put them in motion; but when neither of these causes exist, it is no wonder if they remain at rest; for the voluntary motions do not depend solely on the parts being in a fit state for action, but on an exertion of the power of the will; which does not take place in time of sleep, unless when the *sensorium commune* is disturbed, or the nerves of some part of the body are affected by an unusual *stimulus*. And indeed, there is great reason to believe, that were any of the voluntary muscles as constantly exposed to the action of an irritating cause as is the heart, they would at all times, when we are asleep as well as awake, be agitated

tated with alternate contractions. As a proof of this, many instances might be given of the voluntary muscles continuing to move in time of sleep. Thus, in asthmatic cases, the patients often perform inspiration, when asleep, partly by the action of those voluntary muscles which serve to raise the *scapula* and superior parts of the *thorax*; and there have not been wanting various instances of other voluntary muscles being affected with convulsive motions which did not cease in time of sleep. I shall only mention one that was very remarkable.

A girl of 8 years, for whom my advice was asked, was, without any known cause, seized with an alternate involuntary motion of the temporal and *masseter* muscles, which, altho' it was repeated about 140 times in a minute, and continued without intermission for fifteen days in time of sleep as well as when the patient was awake, yet was attended with no sense of pain, uneasiness, or lassitude in those muscles.

Now, if an unusual irritation of the nerves or fibres of the temporal and *masseter* muscles could produce an alternate motion in them, much quicker than that of the heart in a sound state, and which continued for many days without intermission; why may not the heart, which is at all times exposed to the alternate *stimulus* of the returning venous blood, continue its alternate motions through the whole of life, without either being wearied, or needing any time for repose? And this the rather, since it is not improbable, that, as the heart and intestines retain their power of motion, after they are separated from the body, longer than the other muscles, so their nerves may be framed in such manner as to be better able than those of the voluntary muscles to continue their motion uninterrupted, and without wanting any intervals of rest.

I shall only add, that the account which I have given of the continuance of the vital motions in time of sleep, is rendered still more probable, by observing, that, in a deep sleep, when the heart, as well as the other muscles and organs, loses a little of its sensibility, its motion becomes somewhat slower than when we are awake; and that *opium*, which is known to destroy, in a great measure, the

sentient power of all the parts of the body, has been observed to render the motions of the heart remarkably slow in dogs, frogs, and other animals *.

As the minute structure and distinct uses of the brain and *cerebellum* are little known, I am far from expecting, that what I have now advanced, concerning the cause of the continuance of the vital motions in time of sleep, will give every one full satisfaction, or be thought clear of all difficulties; I have only offered, in a few words, what seems to me most probable, being ready to change my opinion, as soon as farther experiments or observations shall lead us to a better and more consistent account of this matter: “ Sequimur probabiliora; nec ultra quam id quod verisimile occurrit progredi possumus, et refellere sine pertinacia, et refelli sine iracundia, parati sumus †.”

S E C T. XIII.

Of the difference between respiration and the motion of the heart in sleeping and waking persons.

ALTHOUGH the vital motions continue without any interruption in time of sleep, yet as they suffer some change at that time, *i. e.* become then somewhat fuller, slower, and more equable than when we are awake, it may be proper to inquire into the reason of this appearance.

IT has been shewn, that as the dilatation of the ventricles of the heart is owing to the force of the reflux venous blood; so their contraction is produced by the same blood acting upon them as a *stimulus*

* A celebrated author, who allows that *opium* weakens and at length destroys the peristaltic motion of the stomach and intestines, and the moving power of all the other muscles, trusting to two or three inadequate experiments, has been induced to affirm that *opium* does not lessen the irritability of the heart, or render its motion slower or more languid. This mistake I have endeavoured to rectify in the appendix to the 3d edition of my *Physiological essays*, pag. 282—292.

† CICEERO. Disput. Tusculan. lib. 2.

stimulus *: and that the heart can only be affected by *stimuli*, in so far as it is a sentient organ, *i. e.* endued with feeling †. Whence it must follow, that the slowness of the pulse in sleep, and indeed in every other case, can only arise from one or more of the following general causes, *viz.* 1. A diminution of the stimulating quality of the blood; 2. Its slower return to the heart; or, 3. A less degree of sensibility or aptitude for motion in the heart itself.

I. WHEN one has eat or drunk a great deal before sleep, his pulse will be, not slow, but quick and full; because the stimulating power of the blood is increased, by a large quantity of chyle received into it; at the same time that the heart, by its sympathy with the stomach, is often rendered more irritable. Much the same thing may happen from sleeping in too hot an air, or under too great a weight of cloaths: for we know, that heat quickens the circulation of the fluids in all animals. On the other hand, when one has fasted long before sleep, and lies very cool, his pulse will, in time of it, be unusually slow.

BUT when the blood is neither loaded with new chyle, nor become acrid through the want of it; neither too much heated by cloaths or the external air, nor too cool, through want of proper covering; its stimulating quality will neither be augmented nor diminished by sleep; but will continue the same as in a person who is awake in the same circumstances. It remains therefore, that the slowness of the heart's motion in time of sleep, must be owing, either to the slower return of the venous blood to it, or to some diminution of its sensibility.

II. IT is well known, that the affections of the mind disturb the motion of the heart; that the pulse is quicker when we sit or stand than when we lie; and that the action of the muscles of voluntary motion, not only promotes the return of the blood to the heart, but determines it thither with much greater force than usual. In sleep, therefore, where the horizontal posture of the body, the quiescence

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of

* See above, sect. iii. and iv.

† See above, sect. xi. also pag. 142. &c.

of the voluntary muscles, and composure of the mind, all concur to render the return of the venous blood to the heart more equable and slow, the contraction of that organ must be renewed at greater intervals, and with more regularity, than when we are awake, and the circulation is quickened or disturbed by some, or all, of the above-mentioned causes.

BUT, if no farther circumstance, tending to retard the motion of the heart, were found in sleep, the pulse should be equally slow and full in a waking person lying at rest in a horizontal posture, and whose mind is composed, as in the same person in time of sleep; which, however, does not seem to be entirely the case: for though the difference may be small, and there may be many causes which may render it difficult, by observation, to determine that difference with any degree of certainty; yet the remarkable slowness and fullness of the pulse in the deep sleep accompanying an apoplexy, or occasioned by *opium*, makes it probable, that, even in the much gentler sleep of people in health, the pulse is somewhat slower and fuller than it would be, merely from the composure of mind, horizontal position of the body, and quiescence of the muscles of voluntary motion. Let us therefore see whether the slowness and fullness of the motion of the heart in time of sleep may not be, in part, owing to some diminution of the sensibility of this organ.

III. IN time of sleep, as the exercise of the several senses is either suspended or much impaired; so the sensibility or feeling, with which the organs of the body are more or less endued, seems to be rendered less acute. Thus we feel ourselves affected with a kind of *stupor*, when we are just falling asleep, and are then insensible of lesser *stimuli*. The thin rheum, which, by irritating the wind-pipe, keeps us almost perpetually coughing when awake, gives us little or no disturbance in sleep: any unusual *stimulus* in the intestines is also less perceived then; and hence it is, that a purgative taken at night is much longer before it operates than when it is swallowed in the morning. If the heart, therefore, like the other organs of the body, becomes less sensible or irritable in time of sleep, it will not be so quickly

quickly excited into contraction as usual by the venous blood rushing into its cavities; and hence its contractions will not only be more slowly repeated, but the pulse will be full, because the ventricles do not contract, till they are much distended with blood. This will still more clearly appear, if we consider how very slow and full the pulse is in an apoplexy, where the sensibility of all the parts and their aptitude for motion are much more impaired than in common sleep: and how *opium*, which occasions sleep, and lessens the sense of feeling every where through the body, when given in a large dose, renders the pulse uncommonly slow and full.

Does not the slower digestion of the aliment in time of sleep proceed, partly, from the peristaltic motion of the stomach and intestines being then repeated after longer intervals? In dogs who have swallowed a large dose of *opium*, this motion is much lessened or totally suspended, the food last received into the stomach remains indigested, the bowels are more than usually empty, and the lacteal vessels are invisible *. In sleep, therefore, not only the heart but the stomach and intestines also, become less sensible of the *stimuli* usually affecting them, and consequently repeat their contractions more slowly.

DE GORTER, differing from Boerhaave and others, thinks, that the pulse must be, not fuller, but softer in time of sleep; because the circulation of the blood is allowed then to proceed more slowly than when we are awake †. But, from what has been said, it appears, that the fulness of the pulse in sleep is not owing to the quicker circulation of the blood, but to a less degree of sensibility in the heart, whereby its ventricles are not excited into contraction, till they have been more fully dilated than usual by the returning blood. It is, however, to be observed, that the fulness of the pulse in sleep may be owing partly to the fluids passing with greater difficulty through the very small lateral arteries, and the secretory tubes of the glands ‡. For we know that the fulness or softness of the

* Kaau Impet. faciens Hippocrat. dictum. No. 434. 535.

† De Gorter, Exercitat. de somno et vigilia § 40.

‡ Boerhaave Institut. med. § 597.

the pulse does not depend, solely, upon the quantity of blood thrown out by the left ventricle of the heart, but also upon the more or less difficult passage of this fluid through the extreme arteries; since, in proportion as these are obstructed or open, a greater or less resistance will be opposed to the blood projected by the heart.

AFTER what has been said of the slowness of the motion of the heart in sleep, it will be easy to shew why respiration should be performed then at greater intervals.

THE cause exciting the alternate contraction of the inspiratory muscles, is an uneasy sensation in the lungs, occasioned by the blood pushed into their vessels by the right ventricle of the heart *. If then less blood is sent, in a given time, into the lungs in sleep, than when we are awake, the necessity of new supplies of fresh air will be lessened, and consequently inspiration will be performed at greater intervals.

FURTHER, as in time of sleep the sensibility of the lungs, like that of the heart and intestines, must be somewhat impaired, respiration must also, on this account, be performed more slowly; for the inspiratory muscles will not be excited into action till a greater degree of irritation than usual be occasioned by the blood accumulated in the pulmonary vessels. And to this it is owing, that respiration is not only slower but somewhat deeper in time of sleep, than in a waking person at rest in a horizontal position.

IN comatous and apoplectic cases, where all the feelings of the body are much more impaired than in ordinary sleep, respiration is not only much slower and deeper than usual, but, sometimes, after expiration is finished, a pause of 15, 20, 30, or more seconds will intervene before a new inspiration is begun. Much the same thing happens to animals who have swallowed too great a quantity of *opium* †.

Now, if it be reasonable to ascribe the slow, deep, and interrupted breathing, in such cases, to the insensibility which attends those diseases of the head, and which *opium* never fails to produce, when
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* See sect. viii. above.

† See above pag. 104.

taken too liberally; are we not hence led to conclude, that the less remarkable change of breathing which happens in sleep, is owing, partly, to the sense of feeling in the lungs being then somewhat diminished, though in a much less degree than in those morbid cases?

THE whole amounts to this: In ordinary sleep the sensibility of the heart and lungs suffer so small a diminution, that their motions will be very little more affected by it, than they would be from the horizontal position and rest of the body, and composure of mind attending it. In the deeper sleep which succeeds great fatigue, the motions of the heart and lungs will be more observably altered. And, in the most profound sleep, occasioned by *opium* or a morbid state of the brain, where a general insensibility prevails over the whole body, the pulse will become more remarkably slow and full, and respiration slower and deeper.

S E C T. XIV.

Of the motions observed in the muscles of animals after death, or their separation from the body.

SINCE the heart in many animals continue their alternate contractions for some time after they are taken out of the body; and as this is a circumstance which may be offered as a strong objection to the account which we have given of the vital motions *, we

* Constat vero piscium plurimos, nec non insecta, et alia quædam animalcula, motus sui aut vitæ admodum esse tenacia, adeo ut in partes quoque dissecta se aliquamdiu adhuc motitent, imprimis, si adhibito stimulo insuper laceffantur. Quo ipso, luce meridiana clarius apparet motus istos sine sensu edi: quis enim animam sentientem dividi aut sectione multiplicari posse existimet? Peyerii Parerg. anatom. vii. p. 200.

“ Sed manifesto falsum est, motus omnes ab anima oriri, et absque ea materiem fore immobilem segnemque massam. Nam vis contractilis ad stimulum quemcunque, ad quam motus
 “ cordis, intestinorum, et foret omnis motus in homine, pertinet, ne requirit quidem animæ
 “ præsentiam, superest in cadavere, suscitatur mechanicis causis, calore, flatu; neque deserit fibræ,
 “ quamdiu nondum refrigerata riguit, etsi dudum animam abegerit destructio cerebri
 “ cordisque. etsi, ex ipso corpore revulsus musculus, ab omni imaginabili animæ sede separat-
 “ tus sit;” Haller. Prim. lin. physiolog. No 562.

we shall here inquire into the nature and cause of those motions which are so frequently seen in the muscles of animals after death, or their separation from the body; and I flatter myself much, or it will appear, that instead of these being inconsistent with our theory, they will serve rather to illustrate and confirm it.

SEVERAL authors (some of them indeed of great character) have ascribed the motions of the heart after death, or its separation from the body, to some peculiar property, not found in the other muscles, wherewith they suppose it to be endued *: but with what reason, will appear from the following experiments and observations.

1. AN eel, which I dissected, moved the muscles of its body with great force for above half an hour after the removal of its heart and the other *viscera*; and, though I had not leisure to observe them, I doubt not but these motions continued much longer; for Dr Harvey observes, that not only the heart, but the flesh of eels continues to move after being cut in pieces.

2. I have often observed a frog turning from its back to its belly, and leaping about for an hour after the heart and other *viscera* were cut out; and when its muscles were at rest, they have been brought into convulsive contractions, by pricking them with a pin or a pen-knife: nay, the limbs of a frog generally move for some time after they are separated from its body.

3. A tremulous motion has been observed in the muscles upon the *sternum* for a quarter of an hour after it was cut out of the body; and, when it had ceased, it was renewed by pricking the fibres of those muscles with the point of a knife †. The like tremulous motions continued for an hour in the muscle of an ox separated from

* Galen imagined that motion was as natural to the heart as rest to the other muscles: and in his *Administratio anatom.* lib. vii. cap. 8. he concludes, that from the heart's continuing to move after it is taken out of the *thorax*, its motions do not depend upon the nerves or any influence proceeding from them. See also Van Swieten Comment. in Aphor. Boerhaave, vol. 1. p. 2. & 3. And I. G. H. Comment. in Boerh. Instit. med. vol. 5. p. 101. 104.

† Schwencke Hæmatolog. p. 28.

from its body immediately after it was killed, and, upon their ceasing, they were recalled, by pricking its fibres with a sharp instrument *.

4. IN a young pigeon, which I killed by separating its head from the *vertebræ* of the neck, the divided muscles of the left side of the *thorax* (upon which some of the blood thrown out of the heart had been spilt) were agitated with alternate contractions for about ten minutes. These contractions were very quickly repeated at first, but, like those of the heart, became much slower before they stopt altogether.

WERE not the alternate contractions of these muscles more remarkable, and of longer continuance, on account of the *stimulus* of the blood which was spilt on them? This seems not improbable, since, as has been observed above †, the motions of the *vena cava* continue longer than those of the heart, because it is longer supplied with blood. Besides, the alternate contractions which happened to the muscles of the *thorax* in other pigeons which I opened, and where no blood was spilt, were less remarkable, and lasted but a short time.

5. SWAMMERDAM informs us, that, in dissecting animals alive, he observed contractions, not only in every muscle separated from the body, but also in every muscular fibre ‡. And the same kind of motion has been remarked in the muscular fibres of men, which had been cut away in the extirpation of tumors.

6. THE vermicular motion of the intestines remains for a considerable time after they are taken out of the body.

* HENCE it appears, that all the muscles of living animals, whether they be of the voluntary or involuntary kind, are agitated with alternate contractions, after being separated from their bodies; and, consequently, that the vibrations performed by the hearts of ani-

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* Schwencke Hæmatolog. p. 28.

† Sect. vi. No. 3.

‡ Tractat. de respirat. cap. 7. p. 67.

mals, after they are cut out of the *thorax*, bespeak not any latent power peculiar to the fibres of that organ, or which they do not share in common with those of every other muscle.

* If the voluntary muscles, which in a healthful state remain at rest when the will interposes not to the contrary, are alternately contracted and relaxed, as well as the heart, when they are separated from the body; it cannot be concluded, that, because the heart beats after such separation, it must therefore move also while in the body: on the contrary, it follows, that the alternate motions of the heart in living animals must be owing to its being acted upon by some particular cause which does not affect the voluntary muscles.

7. THE heart of an eel, which I cut out of its body, and divided into two parts, continued its vibrations above twenty minutes.

8. WHEN the heart of an eel inclosed in an exhausted receiver, after beating about an hour, had become very languid, and almost ceased from motion, Mr Boyle renewed its pulsations by breathing on that part of the glass where it lay *.

9. I have observed the hearts of frogs beat 12, 15, 18, or 30 minutes and more, after being separated from their bodies; and when their motions began to languish, or were just about to cease, they were increased or renewed by heat, or pricking them with a pin.

10. THE hearts of frogs, which, when first separated from their bodies, beat about sixty times in a minute, performed from betwixt 90 and 100 pulsations in the same time when exposed a little to the heat of the fire; but, after being removed from it, their vibrations became gradually slower, till they were no quicker than at first. While warmth thus increases and renews the motion of the heart, even in those animals whose blood is cold, too great heat destroys

* Philosophical transact. abridged, vol. 2. p. 222.

destroys it both in hot and cold animals, by producing such a change in the nerves, muscular fibres, and their fluids, as renders them unfit for motion. Hence the heart of a pigeon or frog immediately loses its motion when immersed in boiling water.

11. THE hearts of vipers continue their alternate motions for several hours after they are separated from their bodies *.

12. THE heart, of a viper, which beat only 25 times in a minute, when Dr Langrish first took it from its body, was, by the warmth of his hand, soon made to perform 48 vibrations in that time; and, being afterwards put in water a degree or two warmer than human blood, it repeated its pulsations 87 times in a minute †.

13. UPON stretching the neck of a cock so as to separate the head from the *vertebræ* of the neck, several violent convulsions ensued, and in less than five minutes he seemed to be dead. At this time laying the *thorax* open, I observed the heart performing its alternate motions, but more faintly than that of a frog or eel when separated from the body. Three minutes after, when the motion of the heart was become still weaker, I cut it out of the body, and found that its vessels and cavities had been filled with blood; which was no sooner evacuated, than the tremulous motions ceased; nor could they be recalled by breathing upon it, or pricking it in several places with a pin; but, by touching it two or three times with a red-hot iron, a vibrating contraction was observed, which scarcely lasted a second.

14. THE heart of a chick taken out of the shell, beat an hour after its head and breast-bone were clipped off with a pair of scissors, and the auricle retained its motion some time after the heart. The motion of the other parts seemed only to survive the loss of the head for a few moments. The motion of the heart, when about

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* Boyle's Usefulness of experimental philosophy, part 2. p. 16.

† Cronean Lectures on muscular motion, No 150.

to cease, was frequently renewed by pricking it with a pin. In another chick, the heart was kept beating, by the influence of warmth, above two hours after the head was cut off *.

15. I laid open the *thorax* of a young pigeon, four minutes after separating its head from the *vertebræ* of the neck, and found the heart, with its right auricle, which was distended with blood, without any motion. I let a few drops of warm *saliva* fall from my mouth on the heart; upon which its right auricle began to move, and continued repeating its alternate contractions with remarkable vigour and quickness for three minutes, when they became gradually both weaker and slower. At eleven minutes from the beginning of the experiment, the motions of the auricle were still slower, but were quickened somewhat by pricking it with a pin. After eighteen minutes, the contractions of the auricle were more feeble, and not repeated till after 7, 8, or 9 strokes of my pulse; whereas, at first, they succeeded each other more quickly than the vibrations of my heart. Before the twentieth minute was expired, the motions of the auricle ceased entirely; but were so far renewed afterwards, by filling the *thorax* with water of the same warmth with the human blood, as to last about two minutes. During all this time, no motion was observed in the body of the heart; nor were its fibres excited into contraction by pricking them with a pin, seven minutes after the *thorax* was opened.

16. I opened the *thorax* of another pigeon, three minutes after I had pulled off its head, and made a ligature about its neck: the right auricle, with part of the *vena cava inferior* adjoining to it still continued to beat, but the ventricles were at rest: some time after, when the motions of this auricle were about to cease, they were renewed with their former vigour by drawing asunder the sides of the divided *thorax*, and consequently stretching the great vessels leading to the heart. When the motions of the auricle were become languid and slow, the *vena cava inferior* made several contractions before

* Boyle's Usefulness of experimental philosophy, part 2. p. 15. & 16.

before the auricle contracted once; and it continued to palpitate for some time after the auricle had ceased from motion altogether.

17. IMMEDIATELY after separating from the *vertebræ* of the neck the head of a pigeon somewhat younger than either of the two former, I laid open the *thorax*, and found the heart beating pretty strongly, and at every *systole* throwing out the blood with a considerable force by a wound which I had accidentally made in it. When after a few contractions, its motion had become more feeble and irregular, it was made to recover its vigour and propel the blood through the wound as before, by drawing the divided *thorax* a little asunder. Some time after this, when the heart was become more languid, its contractions were renewed with double force as often as I raised the point of it with my finger. About six or seven minutes after opening the *thorax*, the motions of the heart could be perceived only in its right auricle and *apex*, but were most remarkable in the former. In three minutes more, when no motion could be seen in the ventricles, this auricle still continued to vibrate, and its motions were sensibly quickened by raising the point of the heart, and consequently stretching the great vessels adjoining to its base. At this time a few weak palpitations were excited in the heart by dividing it with a sharp knife.

18. SOME students having hanged a cat till she was dead, opened the *thorax*, and observed only a tremulous motion in the heart, which soon ceased, but was renewed by pricking it with a sharp instrument; after this, by squeezing the cardiac nerves downwards, or otherwise irritating them, the heart was made to perform two or three pulsations; which it continued to do for a considerable time, whenever the cardiac nerves were thus stimulated.

19. THE heart of a cat, which had been dead four hours, was excited into alternate contractions, by blowing warm air into its cavities through a tube fixed in the *receptaculum chyli**.

* Wepfer. Hist. cicut. aquat. p. 89.

20. THE motion of the heart was renewed, in the same manner, by Brunnerus, in a dog which had been a good while dead *.

21. EVEN in man, the heart retains a power of motion for some little time after its separation from the body; as appears from the well known account which Lord Bacon gives of the heart of a malefactor, which having been cut out of his body, and thrown into the fire, leapt up several times to a considerable height †.

22. IT is observable, that, after the convulsions which animals suffer at the time of death have ceased, their muscles remain at rest, unless they are stretched, cut, exposed to the air, or otherwise stimulated.

FROM the above experiments it appears,

α THAT the separated hearts of some animals vibrate more strongly, and for a much longer time, than those of others. N° 9. 11. 13.

β THAT animals of the amphibious kind, which have either no lungs or imperfect ones, which bear the air-pump long, and whose blood is cold, as well as languid in its motion, shew signs of life, not only in their hearts, but also in their other members, longer after they are separated from their bodies, than animals which have more perfect lungs, hotter blood, and a quicker pulse. N° 1. 2. 4. 14.

THOSE animals whose parts preserve motion and appearances of life longest after being separated from their bodies, seem to have both their fluids and solids somewhat different from those of other animals: their blood is not only colder, but perhaps more viscid and less volatile; and their fibres are so constituted, that constant supplies of this fluid from the heart are not necessary to keep them in due order for motion; while their nerves, after their communication with the brain has been cut off, preserve their powers much longer than in man and the more perfect animals: thus frogs, eels, vipers, and

* Experiment. circa pancreas, p. 21.

† History of life and death, sect. ix. No. 31.

and tortoises live and move several hours without their heart; and the various parts of their bodies continue to move long after all communication between them and the brain is cut off.

2 THAT, *ceteris paribus*, the heart preserves its motions longer in young animals, after its communication with the brain is intercepted, than in older ones. N° 13. 15. 16. compared with 14. 17.

3 THAT, *ceteris paribus*, the hearts of those animals which continue to beat longest after being separated from their bodies, perform their vibrations at the greatest intervals. N° 9. 10. compared with 11. 12. The reason of this is easily understood; since, as has been just observed, in those animals whose hearts beat longest after separation from their bodies, the blood is coldest and its circulation most languid.

THAT the motions of the heart, after death or separation from the body, are generally more conspicuous, and last longer, than those of the other muscles. N° 14.

4 THAT the right auricle continues to move after the heart appears motionless. N° 14.—17.

5 THAT the *vena cava* and *sinus venosus dexter* preserve their motions still longer than the right auricle. N° 16.

6 THAT the motions of the heart and other muscles, when separated from the body, are not only at all times increased, but even renewed, when they are just at an end, by heat, wounds, stretching their fibres, or any thing else that can gently irritate them. N° 2. 3. 4. 8. 9. 10. 12.—17.

7 THAT, after the heart has entirely, and for a considerable time, ceased to move in animals, it may be excited into action by stretching or stimulating its fibres or nerves, N° 18. 19. 20.

8 THAT as in living animals the voluntary muscles are not convulsed, except when some *stimulus* is applied to them; so in animals newly dead, no convulsive contractions happen, provided the skin be not so cut as to lay them bare, and expose their fibres to some kind of irritation. N° 22.

9 WHENCE it follows, that the vibrating contractions of the muscles of animals after death, or their separation from the body, cannot be owing to any innate power, whereby, independent of all external

ternal causes, they move themselves alternately, but must be ascribed to the action of a *stimulus* of one kind or other upon their fibres.

WHEN the heart is taken out of the body in animals newly dead, the cutting alone must be a considerable *stimulus*; and therefore must not only excite or increase its motions, but also make them continue long. When the *thorax* and *pericardium* are only laid open, the vibrations of the heart will be increased and continued by dissecting and stretching those parts with which it is nearly connected, (N^o 16. 17.), and even by the external air acting as a *stimulus* upon its sensible nerves; for the particles of this fluid are never at rest, but agitated with incessant vibrations. This undulatory motion of the air is so considerable, as to be observed by the assistance of good telescopes; and it is further discovered by the constant whirl of dust and other light substances, in the stillest room, when the sun-beams enter by a small aperture.

THE motions of the heart, therefore, in animals newly dead, or after it is separated from the body, are owing to the *stimulus* of the blood remaining in its cavities, to the contact of the external air, or to the irritation which is communicated to it by stretching or cutting its own fibres, or those of such parts as happen to be immediately connected with it.

IN a *syncope*, and in animals newly dead, the intestines continue their peristaltic motion after the heart has ceased to vibrate, which cannot be ascribed to their being more fitted for motion, since the heart, when separated from the body, or otherwise irritated, moves with more force than they; but is owing to their being acted upon by their usual *stimuli*, even after the heart is deprived of that regular and alternate supply of venous blood which was wont to keep up its motion: as therefore the bile, air, and aliment, remain in the intestines equally after death as before it, they will continue to excite the fibres of the various portions of this canal into alternate contractions, till at length they become insensible and rigid with cold.

MR BOYLE observes, that the heart of an eel being placed in a small receiver, became turgid when he exhausted the air, and beat as sensibly and more quickly than it had done before *; the reason of

* Philosoph. transact. abridged, vol. 2. p. 222.

of which appearance is evident from what has been said: since the stretching of the fibres of the heart thus swelled, must have had the same effect in quickening its vibrations as any other *stimulus*.

DR HARVEY observed, that in time of incubation, the heart of the chick, whose motion languished, and at length ceased in the cold air, quickly recovered its vigour by heat, and contracted with greater force and frequency as often as it was touched with the point of a needle or any thing else that could irritate *; from which it follows that the same causes excite the motion of the heart in living animals and in those newly dead, in the body and out of it.

THE conclusion to be drawn from what has been said is, that there remains in the muscles of animals and their nerves, for some time after death, or their separation from the body, the immediate cause of motion, which may be excited into action, as in living animals, by any *stimulus* or irritation. How or by what means this happens, shall be the subject of our next inquiry.

SOME have ascribed the motion of the heart out of the body, and consequently of other muscles separated from it, to the spirits remaining in their nerves, which, by the capillary attraction of these tubes, or the cold contracting them, continue for some time to go into the muscular fibres. But from such an equable flow of the spirits, it will be difficult to account for the regular alternate contractions and relaxations of those muscles, or for their being excited and renewed by *stimuli* of very different kinds.

OTHERS have deduced the vibrations of the heart, when out of the body, from the elastic power of its fibres †, or of the spirits lodged in them ‡; which are excited into oscillations by any impulse or irritation, and which, observing the same laws with other elastic bodies, must persist in those tremulous motions for some time.

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* De generat. animal. exercitat. 17.

† Hoffman. System. med. tom. 1. lib. 1. sect. 1. cap. 3. No. 18.

‡ Lancisi de corde, prop. 58.; Lieutaud Element. physiolog. p. 71, 72.; Senac Traité du coeur, vol. i. p. 434. and 452.

BUT if the motions of the heart, or other separated muscles of animals, were owing to such a cause, how could their vibrations be excited by bringing a red hot iron near them, after the impulse and tearing of a pin or the point of a knife had ceased to have any effect? (N^o 13.) Will warm water heighten and increase the elastic powers of any body? Does it not rather weaken and relax animal fibres? And how can acrid liquors, which communicate no impulse at all, excite vibrations in an elastic machine?

FURTHER, as the times of the vibrations of a *pendulum* in a cycloid would be exactly equal, however unequal the arches which the body describes may be, were it not for the small inequality that the resistance of the air occasions; so the vibrations of a *pendulum* in a small arch of a circle, which coincides with the cycloid, and the oscillations of elastic bodies, would follow one another at equal intervals of time, were it not for the air, which, as it resists a great vibration of a *pendulum* or elastic body more than a small one, must consequently retard it more; whence, strictly speaking, the first and greater vibrations of such bodies must follow each other more slowly than the last and smaller ones. But as this difference is too inconsiderable, especially in small vibrations, to be perceived by us; so, in a physical sense, we may be allowed to say, that the vibrations excited in elastic bodies by any external cause, though they be always decreasing in greatness and force, are yet performed from first to last at equal intervals of time. Let us now see how far the separated hearts of animals observe the same law, in their motions.

23. THE hearts of frogs, when their *thorax* is first laid open, generally beat about sixty times in a minute; but after they are separated from the body, and have been in motion for some time, their vibrations begin to grow sensibly slower, so as only to be renewed after an interval of 2 or 3 seconds; and, a little before their motion ceases altogether, I have counted 7, 10, 14, 15, 16, or more beats of my pulse * between their pulsations, each succeeding pulsation following the former not till after a longer pause, which at

* The motion of my pulse was at the rate of 75 in a minute.

at last ended in a final stop. In the separated heart of a frog (into whose stomach I had forced, about an hour and a half before opening it, a small quantity of *opium* dissolved in water) I observed the intervals between the six last pulsations to increase nearly in the following proportion, 11, 13, 16, 19, 23, 30; which numbers denote how many beats of my pulse intervened betwixt each of these vibrations. From which it appears, that, before the last pulsation of this frog's heart, there was a pause of 24 seconds.

WHAT is here said of the motions of the separated hearts of frogs becoming slower as they grow weaker, is also true of the hearts of eels; and holds not only in the pulsations of the right auricle of the heart of a pigeon, which remained in the body after death, (N^o 15.), but in the contractions also of the muscles of its *thorax* after dissection, N^o 4.

SINCE, then, the motions of the hearts of animals after death, or after they are separated from their bodies, decrease gradually in quickness, as well as in strength, and become, at last, so slow, that before they cease altogether, the heart reposes itself, as it were, for some time, and, after appearing to be dead, performs yet another contraction slowly, and with much seeming difficulty; it follows, that those motions are regulated by laws different from those of elastic bodies.

WE may also see, from what has been said, with how little reason the motion of the heart, after its separation from the body, has been ascribed to the alternate action of its distending fluids and contracting solids; and compared to the *follis luforius*, which being let fall from a height, does not lie still upon the ground, but is immediately thrown off from it, and continues to rise and fall alternately for some time*.

IT appears, from the experiments already recited, compared with N^o 8. 9. 10. and 11. of Sect. I. and with what has been advanced Sect. X. p. 128.—130. and 135.—137. that the motions of the heart and other muscles after death, and when separated from the body,

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* Santorini de structura et motu fibræ, sect. 73.

are owing to a *stimulus* ; that where no *stimulus* is applied, they either happen not at all, or soon cease ; that, when failing, they are excited a-new by any irritation ; and that, in the laws which they observe, and the appearances which they exhibit, they agree exactly with the motions which *stimuli* excite in the muscles of living animals. But we have fully shewn, in Sect. X. that the contractions of the muscles of living animals, arising from any thing that tears, stretches, or otherwise irritates their fibres, are not owing merely to the peculiar structure and arrangement of their parts as mechanical organs, or even to the sole efficacy of any material powers, but to their being endued with feeling, and animated by a sentient principle. Whence it follows, that the motions of the heart and other muscles, after death, or their separation from the body, must proceed from their sensibility. As long as this sentient power remains, or is but little impaired, they are impatient of any irritation, and are, therefore, alternately contracted and relaxed ; but when it becomes considerably weaker, stronger *stimuli* are required to rouse them into action, and even then their motion is more languid.

HARVEY, who formed his judgment of things, not as imagination might suggest, but from repeated experiments and observation, ascribes the various and irregular motions of the heart of the chick, when irritated by different *stimuli*, to its being endued with sense * ; and therefore compares it to an animal which lives, moves, and feels.

THE motions of the heart from *stimuli* greatly resemble the alternate contractions of the *panniculus carnosus* of brutes, when their skin is tickled or stung by insects : and as this muscle cannot properly be considered as a mere mechanical organ, but as something animated, which endeavours to throw off whatever affects the surface of the body with any disagreeable sensation ; so the motions of the separated hearts of animals are not to be ascribed to any property they can be possessed of as mere material organs, but to their being still endued with some kind of life and sense, which makes

* De generat. animal. exercitat. 17.

makes them shew an impatience of whatever hurts them, and endeavour, by their alternate contractions, to throw it off.

24. AT eleven o'clock in the forenoon, I injected a solution of *opium* in water into the stomach and intestines of a frog, by the mouth and *anus*. In less than a quarter of an hour, it had lost a good deal of its vivacity and power of motion, and, when touched or pricked, it dragged its limbs as though their muscles had been in some degree paralytic. In little more than half an hour, it seemed to have lost all power of motion. At two in the afternoon when I opened it, I found the auricle of the heart, with the large vessels attached to it, distended with blood; but there was no motion either in the heart or its auricle: nay, so very insensible were these parts become of any *stimulus* or irritation, that neither tepid water, nor pricking or tearing their fibres, had any influence in exciting them into motion. Hot water, indeed, being poured into the *thorax* and *abdomen*, made the heart and intestines suddenly shrink and contract, in the same manner as the flesh of any dead animal does when thrown into boiling water; but produced no alternate contractions like those which follow the action of *stimuli* upon the muscles of living animals, or of such as are newly dead. Afterwards, I cut off the head of this frog, and with the point of a probe pressed and broke down the spinal marrow into a pulp; but did not observe the least motion or convulsion in any part of the body.

25. AT half an hour past one in the afternoon, I injected a solution of *opium*, as above, into another frog, and opened it an hour after. The auricle and great vessels leading to the heart were more than usually filled with blood, but not so much as in the last experiment. The heart still continued its motions, but much more slowly than in a sound state: its pulsations followed each other after an interval of about $3\frac{1}{2}$ seconds: the distended auricle always contracted first, and, after it, the ventricle. No convulsions happened in any part of the body from irritating the spinal marrow,
nor

nor were any of the muscles of the limbs or trunk brought into contraction by pricking or tearing their fibres.

26. I forced down into the stomach of another frog a smaller quantity of a solution of *opium*; and, upon opening its *thorax* an hour and three quarters after, I found its heart beating regularly, but as slowly as in the last experiment: when I cut it out of the body, and laid it on a plate, it renewed its pulsations faster, *viz.* once in two seconds; but, after five or six minutes, they became as slow as at first.

α SINCE, from these experiments, it appears that *opium*, internally applied, soon renders the motion of the heart in frogs three or four times slower than it naturally is, and, at length, puts an end to it entirely, so that the causes which use to renew it, prove quite ineffectual for that purpose; and since *opium* received into the stomachs of animals is well known to destroy the sense of feeling, either in the whole, or in part, as its dose is greater or less; is it not probable, that *opium* stops or retards the motions of the heart, only as it renders it wholly, or in a great degree, insensible of the *stimulus* of the returning venous blood; and that the contractions of the heart, both in the body and after it is separated from it, are owing to the sentient power of its nerves and fibres, by which it is made capable of being properly affected by various *stimuli*?

β As the heart continued to beat after the muscles of the trunk and limbs were no longer affected by any irritation; it follows, either that its nerves and fibres are endued with a higher degree of sensibility than those of the other muscles, or, at least, that their sensibility is not so soon destroyed by the *laudanum*.

γ DR Kaau has observed, that the convulsive motions which were excited by irritating or breaking down the brain of a dog to whom he had given six grains of *opium*, were much less remarkable than those he had observed in other dogs who had got nothing to lull their senses *; and experiment 25. above, shews, that no convulsive contractions are produced, either by irritating the muscles themselves,

* *Impet. faciens*, No 435.

selves, or the spinal marrow of a frog, an hour after a solution of *opium* is injected into its stomach and intestines: from which this inference is obvious, *viz.* that convulsions excited in dying animals, or such as are newly dead, by dissecting the spinal marrow, pressing it with a probe, or breaking down the brain, are solely owing to the sensibility of these parts to any irritation, and not to the spirits being mechanically propelled through the nerves into the muscles, either by the cut vessels of the *medulla* or brain retracting themselves *; or by the pressing power of the probe or dissecting instrument applied to them.

IT has, for many years, been the prevailing opinion, that *opium* produces its first and most remarkable effects on the body, not by mixing with the blood, but merely by its immediate action on the nervous *papillæ* of the stomach, whence the brain and whole nervous system are affected. In support of this opinion, many arguments have been advanced, some of greater, others of lesser weight: but the following experiments seem to put the truth of it beyond doubt.

27. A frog continued moving its limbs, turning from its back to its belly, and leaping about for above an hour after I had cut out its heart; and was not quite dead after two hours and a half.

28. FIVE minutes after taking out the heart of another frog, I injected a solution of *opium* into its stomach and intestines. In less than half an hour it seemed to be dead, and neither pricking, tearing, nor cutting its muscles caused any contraction in them, or any motion in the parts to which they belonged. A probe thrust into the spinal marrow, after cutting off its head, made its fore-legs contract feebly.

SINCE, in this frog which was deprived of its heart, the parts of the *opium* could not possibly be mixed with the mass of blood, or be conveyed along with it to the brain, their effects must necessarily be deduced from their direct action upon the nerves and fibres of
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* Kaau Impet. faciens, No 333.

the organ to which they were immediately applied *. But to return from this digression.

SINCE the sensibility of our fibres is owing to their being animated by a living principle different from matter, and of powers superior to it, (Sect. X.), it may be objected, That if we ascribe the motions of the muscles after death, or their separation from the body, to their being endued with sense, we must not only suppose the soul to continue present with the body after death, but also to be extended and divisible.

BUT though these objections, as they are founded in our ignorance of the nature of the soul, and its union with the body, and of the manner of their mutual action upon each other, ought perhaps to have little regard paid them in a physical inquiry; yet, to clear our subject as much as I am able of all difficulties, we shall consider them particularly.

* I think it is not only probable, but even demonstrable, that the soul does not immediately leave the body upon a total stoppage of the motion of the heart, and, consequently, of the circulation of the blood, *i. e.* upon what we usually call DEATH †, but continues for some time present with it, and ready to actuate it. Thus, a variety of insects, bats, hedge-hogs, and other animals, which continue in a death-like state in the cold winter-season, are restored to life by the kindly warmth of the returning spring, which, as it stimulates the solids into contraction, as well as rarifies and agitates the fluids, gives the latent soul an opportunity of shewing itself by its effects: yet, in these animals, during the cold weather, there is no circulation of the blood; they are without feeling, may be torn and cut in pieces, without shewing any sense of pain, and cannot be distinguished from such animals as are really dead, except in this circumstance, that, by the assistance of warmth, they may at any time be brought to life.

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* For a further account of the action of *opium*, see Edinburgh Physical Essays, vol. 2. art. 20.; and Physiolog. Essays, edit. 3. p. 182.

† By death is here meant the general death of the body as a system, and not the particular death of the several parts, which does not happen for some time after.

IN the northern countries, magpies and other smaller birds, after being frozen by the excessive cold, have been soon brought to life again by warmth *; nay, several of the human kind have been recovered by shaking their bodies, blowing into their lungs, or exposing them to heat, after having been for hours, nay, sometimes days, to all appearance, dead, without pulse, breathing, or any degree of natural heat. Had not the soul been present with such bodies, and ready to actuate them, is it to be imagined, that blowing air into the *anus* or lungs, that heat, friction, or any other *stimuli*, could, as it were, by some magic charm, have called it back from distant regions? Upon the whole, it appears certain, that after death, or an entire stop of all motion in the bodies of animals, the soul still remains present with them, and can be again brought to exert its influence, by various kinds of *stimuli* applied to their different parts. May not then the same principle continue present with the several muscles after they are separated from the body, and be the cause of their motions when irritated? And is it not reasonable to think, that the renewal of life in a frozen magpye, and of motion in the frozen heart of a salmon †, by exposing them to the heat of a fire, was owing to the same cause, *viz.* to the living or sentient principle, which being present with the body of the magpye, and the separated heart of the salmon, was excited by the *stimulus* of heat to put them in motion? But here it will be said, that, not only contrary to the opinion of many philosophers, we suppose the soul to be extended, but also, in opposition to them all, seem to make it divisible; which is the second objection mentioned above, and to which I now proceed to give an answer.

As the schoolmen supposed the Deity to exist in every *ubi*, but not in any place; so they imagined the soul of man not to occupy space, but to exist in an indivisible point. Yet, whoever considers the structure and appearances of the animal frame, will soon be convinced that the soul is not confined to an indivisible point, but must be present at one and the same time, if not in all the parts of the body where the nerves are found, yet, at least, at their origin; *i. e.*

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* Flora Siberica, prae fat. p. 73.

† Peyer. Parerg. anatom. 7. p. 200.

it must be, at least, diffused along a great part of the brain and spinal marrow. Nay, while, in man, the brain is the principal seat of the soul, where it most eminently displays its powers, it seems to exist or act so equally through the whole bodies of insects, and other animals of the lowest class, that its power or influence scarce appears more discernible in one part than another: and hence it is, that, in such creatures, the several parts of the body live longer after being separated from each other, than they do in man and the other animals more nearly resembling him, where the soul seems chiefly to act on the different parts by means of their connection with the brain and spinal marrow; or, at least, where the cutting off such connection, soon renders the parts unfit to be any more acted upon by it.

IT was not, therefore, altogether without reason, that some of the greatest philosophers of the last and present age supposed the soul to be extended *.

BUT if the soul, without extension, be present at one and the same time in different places of the brain; and if, in many animals, it can act along the spinal marrow for a great while after the head is cut off, why may not it also actuate parts separated from the body, without being extended? On the other hand, if we allow the soul to occupy space, I do not see why it may not continue to be present with the parts of its body after they are separated, as well as when they were united. And with respect to the divisibility of the soul, which is generally thought to follow from the supposition of its being extended; why may it not be a substance so perfectly and essentially one, as that a division or separation of its parts would necessarily infer a destruction of its essence? Further, if the soul can be present in all or in any considerable part of the body at one and the same time without being discernible, its sphere of existence being
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* Gassendi, Dr Henry More, Sir Isaac Newton, Dr Sam. Clark.

Gassendi argued for the soul's being extended in the following manner. If it be said, that the soul resides in a point of the brain, this is either physical or mathematical; if physical, the difficulty still remains, because this is extended, and consists of parts, and consequently the soul must be extended which occupies it: if mathematical, which has no dimensions, how can the nerves, which are not mathematical lines, all terminate in that which hath neither length, breadth, nor thickness. Gassend. Object. contra meditat. Descartes, p. 32. 33.

so much increased, as to act upon the parts when separated, will not infer its divisibility. As the Deity is every where present, and, in the infinitely distant parts of space, actuates at the same time a vast variety of different systems, without any inconsistency with his unity or indivisibility; so, may not the souls of animals be present every where in their bodies, actuating and enlivening, at the same time, all their different members? Nay, further, when the fibres and threads connecting some of these parts are divided, may not the soul still act in the separated parts, and yet be only one mind?

IT must be owned, that there is a great deal of difficulty and obscurity in these matters. But what hypothesis can we embrace that will clear us of them, or to what part of nature can we turn our inquiries where we shall not find something to puzzle us, some mystery at last which we cannot unfold? Nor is this to be wondered at; since, in the present state, our knowledge is much limited, and we have only access, as it were, to see the surface of things! But because we cannot explain fully, are we therefore in noways to attempt explaining the operations of nature? Because, in accounting for the spontaneous motions of animals, and shewing their dependence on the soul, there occur some difficulties with respect to the nature of an immaterial substance, its manner of existing, and way of acting upon, or being present with the body; are we therefore to deny the reality, influence, and action of this principle, which, from a variety of arguments, appear so evident? At this rate, we ought to give up all inquiry into the works of nature, and, with our arms across, sit down contented in ignorance.

BUT, not to perplex ourselves longer with metaphysical difficulties, we shall recite a few experiments and observations, from which we are led, by analogy, to conclude, that the motions of the separated parts of animals are owing to the soul or sentient principle still continuing to act in them.

29. A frog lives, and moves its members, for half an hour after its head is cut off*; nay, when the body of a frog is divided in two, both the anterior and posterior extremities preserve life and a power of motion for a considerable time.

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* Kaau Impet. faciens, No 331.

30. A young cock, whose head Dr Kaau suddenly cut off with a sharp razor, as he was running with great eagerness to his food, went on in a straight line 23 Rhinland feet, and would have gone farther had he not met with an obstacle which stopt him*. The story, therefore, mentioned by Lord Verulam, of an ostrich running along the stage after its head was struck off with a forked arrow by one of the Roman Emperors, is not improbable†.

31. A viper, after being deprived of its head and intrails, moved towards a heap of stones in a garden where it used to hide itself‡.

32. THE bodies of vipers not only move two or three days after they have been deprived of their skin, head, heart, and other bowels, but are also manifestly sensible of punctures, by means of which they may be made to move with greater vivacity§.

33. THE female butterflies into which silk worms have been metamorphosed, not only admit the male, after losing their heads, but also lay eggs.

34. REDI informs us, that a land tortoise, whose brain he extracted by a hole made in its scull, in the beginning of November, lived to the middle of May following. Immediately after the loss of its brain, it shut its eyes, nor ever opened them any more, but continued to move and walk about until the time of its death. When the scull was opened, its cavity appeared clean and smooth, and nothing was found in it except a small dry clot of blood. The same experiment he repeated on various other tortoises, some of which lived a longer, others a shorter time, but none of them less than fifty days**.

35. A large tortoise, whose head Redi cut off, allowing the blood to

* Kaau impet. faciens, No 331.

† Sylva sylvarum, on the word *life*.

‡ Kaau Impet. faciens, No 331.

§ Boyle's Usefulness of experim. philos. part. 2 p. 16.

§ Ibid.

** Observation, circa animal, vivent. p. 209. & 210.

to flow freely from the open vessels of its neck, lived twenty-three days; and though it did not walk about like those which were deprived of their brain, yet as often as its fore or hind feet were pricked, it moved them with force, and was convulsed. In two tortoises which he opened fifteen days after decollation, he saw the heart beating as in a living animal, and the blood circulating through it *.

HERE, we are naturally led to observe, that while those animals who have a small brain and large spinal marrow, live long after decollation; man, and most quadrupedes, which have a large brain, survive the loss of it only for a few moments.

" IF the motions of a tortoise, after decollation (35.), or the loss of its brain (34.), cannot proceed from mere mechanism, but must be undoubtedly ascribed to the living principle which was the cause of its motions in a sound state; and, if the same is true of the actions performed by butterflies after the loss of their heads (33.); it must follow, that the motions and other signs of life which are observed in the body and limbs of a frog for above half an hour after its head is cut off (29.), are to be attributed to the sentient principle, to which its motions and actions were owing when in an entire state; and if so, then the motions of this body, when divided into two parts, must also be referred to the same cause, since they are of a like kind, although of shorter duration. Shall we then deny that the motions of its separated heart and limbs, which are similar to these, and are increased and renewed by the application of the same causes, proceed from the sentient principle still acting in these parts?

THIS would be to neglect the strongest analogy; and we should be the more inexcusable, as no other cause has hitherto been assigned, so well accounting for these appearances †.

^β WE have no other way to satisfy ourselves that an animal is alive,

* Observat. circa animal. vivent. p. 212. 213.

† A learned and ingenious author who supposes, with Stahl and others, that the motion of the heart is continued and regulated by the soul acting, as a wise and rational agent, with a view to the good of the body, ascribes the alternate contractions of the heart, after its connection, by means of the nerves, with the brain is cut off, to some power depending on its mechanical

live, or endued with feeling, but by observing, whether it shews an uneasiness when any thing hurts, or tends to destroy any of its parts, and an endeavour to remove or avoid it. Since therefore the bodies of vipers make just the same kind of motions when pricked with a sharp instrument, two or three days after losing their head, heart, and other *viscera*, as if they were entire (32.), we are naturally led to conclude, that they are still, in some sense, alive and endued with feeling, *i. e.* animated by a sentient principle. And as the muscular parts of these creatures move after being cut in pieces, and are sensible of punctures, it also follows, that they continue still to be animated *.

Lastly,

mechanical construction. But if the motions of a viper's heart for three days after its head has been cut off, and those of the heart of a tortoise for six months after the loss of its brain, may be owing to a mechanical power resulting from their particular structure, why may not the motions of the heart in these as well as all other animals, from the beginning to the end of life, be owing to mechanism alone?

Indeed, there cannot be a stronger argument of the weakness of the Stahlian account of the motion of the heart, than that one of its ablest defenders has been reduced to account in such a manner for the motions of that organ after its connection with the brain is cut off, as in fact to give up at once all that he had advanced to prove the necessity of calling in the power of the mind to explain the vital motions.

* It has been objected, that the separated members of animals, though they continue to move for some time, are nevertheless neither animated nor endued with any kind of sensibility, otherwise the animal to which those members belonged ought to feel pain when they are irritated.

As I have given an answer to this objection elsewhere (*a*), I shall only observe here, that since the nerves, which are continuations of the medullary substance of the brain and spinal marrow, have been proved to retain their powers of feeling, and of putting the muscles in motion, for some time after their connection with the brain is cut off (*b*), we have reason to believe that the motions excited by *stimuli* in the muscles of animals, after they are separated from their bodies, are owing to some kind of feeling or simple sensation (such as oysters and other animals of the lowest class, who have no brain, are endued with) in those muscles or their nerves, which, though not attended with any reflex consciousness, a power the soul only exercises in the brain, is nevertheless the immediate occasion of all those motions which arise from the irritation of the fibres of the muscles, whether they be connected with the other parts of the body, or newly separated from them.

And here it is proper to remark, that, while those motions which are occasioned by stimulating the fibres of any muscle, continue for some little time after its communication with the
brain

(*a*) See my *Physiological Essays*, edit. 3. appendix, p. 259. and 260.

(*b*) *Ibid.* p. 245. &c.

* *Lastly*, If the motions of the muscles in the limbs of a cock, after decollation (30.), are, without dispute, owing to its soul; may we not also ascribe to the same principle the like, but less remarkable motions in men and quadrupedes, after their heads are struck off; and, consequently, the tremulous motions and palpitations of their hearts too, after death or separation from their bodies?

To sum up all in a few words; from what has been said, it appears evident, that the involuntary motions of living animals, and the alternate contractions of their muscles, after the general death of the body, or their being separated from it, are owing to one and the same cause, *viz.* an irritation of their fibres or nerves, or of such parts as are nearly connected with them. If then, as we have shewn (Sect. X.), that the motions of animal fibres, from a *stimulus*, most certainly bespeak a feeling, and cannot be explained unless we admit it; and if feeling be not a property of matter, but owing to a superior principle, it must follow, that the motions of the heart, and other muscles of animals, after being separated from their bodies, are to be ascribed to this principle; and that any difficulties which may appear in this matter are owing to our ignorance of the nature of the soul, of the manner of its existence, and of its wonderful union with, and action upon the body.

C O N C L U S I O N.

AS philosophical inquiries, however agreeable and entertaining they may be to the mind, become still more interesting when they can be applied to practice; I intended to have shewn, how far the theory of the vital and other involuntary motions, which we have endeavoured to establish, may be useful towards explaining the nature of several diseases, and consequently towards pointing

brain, by means of the nerves, has been cut off; such motions as proceed from sympathy, and are owing to the irritation of some distant part, cease as soon as the brain is rendered unfit for action, or the communication with it is interrupted, because they depend on a perception in that organ from which the nerves proceed, and where alone the cause of their sympathy is to be found.

pointing out the most proper method of curing them. But, as this Essay has swelled to a much greater bulk than I at first expected, I shall now, omitting that part of my design, conclude with a reflection of a different nature.

FROM what has been offered, then, in the preceding pages, it may appear, how unjustly the study of physic has been accused of leading men into scepticism and irreligion. A little philosophy may dispose some men to Atheism; but a more extensive knowledge of nature, will surely have the contrary effect. If the human frame is considered as a mere corporeal system, which derives all its power and energy from matter and motion; it may, perhaps, be concluded, that the immense universe itself is destitute of any higher principle: but if, as we have endeavoured to shew, the motions and actions of our small and inconsiderable bodies, are all to be referred to the agency of an immaterial principle; how much more necessary must it be to acknowledge, as the author, sustainer, and sovereign ruler of the universal system, an incorporeal Nature every where and always present, of infinite power, wisdom, and goodness, who conducts the motions of the whole by the most consummate and unerring reason, without being prompted to it by any other impulse, than the original and eternal benevolence of his nature!

Nam quis non videt, finitæ si breve corpus
Subjicitur menti, mens quanta sit illa supremo
Quæ regit arbitrio vastum quem condidit orbem?
Non poterit sine consilio tam parva moveri
Machina, tam fragilis; te judice, tanta regetur
Mentis inops! Credant Epicuri de grege porci *.

THE true physiology, therefore, of the human body, not only serves to confute those philosophers who, rejecting the existence of immaterial beings, ascribe all the *phænomena* and operations in nature to the powers of matter and motion; but, at last, like all other sound philosophy, leads us up to the first cause and Supreme Author of all, who is ever to be adored with the most profound reverence by the rational part of his creation.

PHYSIO-

PHYSIOLOGICAL

ESSAYS.

CONTAINING,

I. An INQUIRY into the Causes which promote the CIRCULATION of the Fluids in the very Small Vessels of Animals.

II. OBSERVATIONS on the SENSIBILITY and IRRITABILITY of the Parts of Men and other Animals ; occasioned by M. DE HALLER's late Treatise on these Subjects.

PART I.

The first EDITION published in the Year 1755, the 2d in 1759, and the 3d in 1766.

PART II.

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I.

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INTRODUCTION

CAUSES AND EFFECTS

THE CIRCULATION OF THE BLOOD

IN THE

VERTEBRATE ANIMALS

BY



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ALTHOUGH the circulation of the blood has been almost universally acknowledged for above a century past, and much has been written in order to explain that doctrine; yet there are several things relating to it which have not been, hitherto, accounted for in so satisfactory a manner as to render any farther inquiry into them superfluous: and of this kind, we presume, is the motion of the fluids in the smaller vessels.

THE first authors who embraced the Harvean doctrine seem to have ascribed the whole of the circulation, both in the arteries and veins, to the force of the heart*. But Borelli, in whose time it was believed by many, that the arteries and veins were not continued canals, but divided by an intermediate spongy substance, plainly saw, that, in this case, the blood could not be conveyed in-

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* Jo. Walaei epist. ad Bartholin. De motu chyli et sanguinis.

to the orifices of the nascent veins, by any force of the arterial fluids pushing it forwards; and, therefore, he supposes it to enter them in the same manner as the particles of water insinuate themselves into a sponge or other porous substance: but as, in his days, the properties of capillary tubes were little known, it is no wonder that, after declaring attraction to be an impossible thing, he ascribes the above effects to the gravity of the fluid itself*; nor does he seem to have been sufficiently aware, that, after water has risen to a certain height in small tubes, or a certain quantity of it has been received into porous bodies, no more of it will enter into either.

PITCAIRN, in his *Dissertatio de circulatione sanguinis per vasa minima*, after shewing that animal secretion cannot be performed by means of ferments in the glands, or by the glands acting as filters, endeavours to prove, that the various secretions from the blood are intirely owing to the different diameters of the secretory vessels: but he makes it no part of his inquiry, by what powers the fluids are pushed through these vessels. However, that there might appear no difficulty in the motion of the fluids through even the smallest tubes of the body, nor any suspicion of their stagnating in them, we have lately been told, that the blood moves more quickly in the smaller than in the larger vessels; an assertion so inconsistent with the laws of hydraulics, when applied to the animal frame, that it could scarcely have been expected to have dropt from the pen of Hoffman †.

BUT, how easy soever it may have appeared to some authors to account for the motion of the fluids in the small vessels of animals, yet whoever considers the resistance that a fluid, moving through the *aorta* and all its branches, must meet with from friction, which increases as the diameters of the vessels decrease, and adds to this the mutual attraction and cohesion between the particles of the fluids and the sides of the vessels in which they move, will not only see that there is some difficulty in this matter, but

suspect

* Borelli De mot. animal. pars 2. prop. 32.

† Frederic. Hoff. Syst. med. l. 1. § 1. c. 6. No. 17.

suspect that neither the force of the heart, nor the alternate contraction of the larger arteries, is sufficient to drive the fluids thro' the smallest vessels of the brain, *testes*, and many other parts of the body.

IN order, however, to set this affair in a clearer light, we shall consider the several causes to which the circulation of the blood has been commonly ascribed.

S E C T. I.

Of the force of the heart, contraction of the arteries, gravity, and the attraction of capillary tubes, considered as causes of the circulation of the fluids in the small vessels of animals.

AS the principal cause which moves the blood through the body, is the contraction of the heart, let us first inquire how far this may be supposed sufficient to account for the motion of the fluids in the very small vessels of animals.

IF the force with which the blood is thrown, by the left ventricle of the heart, into the *aorta*, be supposed equal to the pressure of a column of blood 90 inches high *; the *momentum* of this fluid in any artery will be found, by multiplying the *area* of the transverse section of that artery into 90, the height of that column of blood whose pressure is supposed equal to the protrusive force of the heart: for the product gives the number of cubic inches or parts of a cubic inch of blood, whose weight is equal to the pressing power with which the blood is driven by the force of the heart into that artery.

THE diameter of a circulating red globule of blood has been generally reckoned something less than $\frac{1}{3000}$ part of an inch; but Martine has, from Leeuwenhoek's and Jurin's later observations, shewn

* The Reverend Dr Hales, from a variety of experiments made on horses, dogs, sheep, and other animals, thinks it probable, that the blood would rise seven feet and an half, or 90 inches, in a tube fixed into the carotid artery of a middle-sized man. Statical essays, vol. 2. p. 40.

shewn it to be $\frac{1}{1933.5}$ part of an inch *; and Leeuwenhoek has observed, that one of these globules is sometimes in passing through a very small capillary artery, forced to change its figure into an oblong spheroid, so that the diameter of such an artery may be supposed nearly equal to that of a red globule. If then, for the sake of more easy computation, we suppose the diameter of a red capillary artery to be equal to $\frac{1}{2000}$ part of an inch, the *area* of its transverse section will be 0.000 000 196, and this multiplied by 90 gives 0.000 0176 parts of a cubic inch of blood, which amounts to 0.00466 or $\frac{1}{214}$ part of a grain †; and is equal to the *momentum* of the blood, arising from the pressing force of the heart, in a capillary artery, whose diameter is $\frac{1}{2000}$ part of an inch, upon the supposition that there were no loss of motion from friction, and that the areas of the transverse sections of all the capillary arteries in the human body were equal to that of the *aorta*: but since this is not the case, and the areas of the former greatly exceed that of the latter, the *momentum* of the blood in a capillary red artery will fall very much short of our computation.

To illustrate this; let us suppose a pipe A of an inch diameter, to be divided into several branches, and at last to terminate in 10000 small tubes *a, a, a, a, &c.* each $\frac{1}{2000}$ part of an inch in diameter; the sum of the areas of whose transverse sections is equal to that of A. If a fluid be pushed thro' such a system of vessels, with any given force, the velocities in the small tubes *a, a, a, a, &c.* will be equal to the velocity in A; and their *momenta* *m, m, m, m, &c.* all taken together, will, bating friction, be just equal to the *momentum* M in the large trunk A, *i. e.* $m \ 10000 = M$ or $m = \frac{M}{10000}$. But if another pipe B of the same diameter with A be divided so as to terminate in 300000 small tubes *b, b, b, b, &c.* each $\frac{1}{2000}$ part of an inch diameter; then, altho' a fluid be pushed thro' the two trunks A and B with the same velocity, and consequently the *momentum* in them be equal, yet

* Edinburgh Medical essays, vol. 2. art. 7.

† A cubic inch of warm blood is reckoned by some 266, and by others a little more than 267 grains: but Martine seems to have fixed it pretty accurately at $264\frac{1}{2}$; and, for the sake of even numbers, I have supposed it to be 265 grains.

yet the velocity in any one of the small tubes $a, a, a, \&c.$ will be to the velocity in any one of the corresponding tubes $b, b, b, \&c.$ as 30 to 1, and consequently their *momenta* will be as 900 to 1.

DR KEILL having, by measuring the arteries of the human body, fixed the proportions of the branches to their trunks after every division, lays down a method for calculating in what degree the velocity of the blood in the different arteries is affected by the increase of the capacity of the vessels through which it flows *: according to this computation, it will be found, that the velocity of the blood in an artery whose diameter is $\frac{1}{2000}$ part of an inch, ought to be to its velocity in the *aorta*, as 1 to 345; and consequently the *momentum* of the blood in such an artery must be $345 \times 345 = 119025$ times less than we have computed it above, *i.e.* $= \frac{1}{2000} \times 119025 = \frac{1}{16.8}$ part of a grain. And since a globule of red blood weighs nearly $\frac{1}{50000}$ part of a grain †, it follows, that the *momentum* or pressing force of such a globule in its capillary artery, arising from the impulsion of the heart, does not exceed twice its own weight.

BUT even this *momentum*, however small it may appear, must be diminished by friction: the precise quantity of which, although it may perhaps be difficult precisely to determine; yet that it must be considerable, will appear from what follows.

1. IF two pipes of equal lengths, whose diameters are $\frac{3.72}{1000}$ and $\frac{90}{1000}$ parts of an inch, be, one after another, screwed into the side of a vessel at the perpendicular distance of four feet from the top of the water, and laid parallel to the horizon, the large pipe will discharge 179, and the small pipe $6\frac{1}{2}$ ounces of water, in half a minute. Hence the velocities of the water in these two pipes must have been as 1293 and 756; and, were it not for the inequality of the resistance of the air, the velocity in the large pipe would have been still greater, and the velocities in the two pipes pretty nearly as the square-roots of their respective diameters ‡.

HENCE, if we could suppose a capillary artery, of $\frac{1}{2000}$ part of an

* Keill's Tentamen, med. phys. 2.

† Edinburgh Medical essays, vol. 2. art. 7. § 11.

‡ Robinson's Animal œconomy, prop. 1. exp. 2.

an inch diameter, to go off directly from the beginning of the *aorta*, without any intermediate branchings, the velocity of the blood in it would be (*cæteris paribus*) to the velocity of the blood in the *aorta*, nearly as $\sqrt{\frac{2}{0.0005}}$ the diameter of the capillary is to $\sqrt{\frac{2}{0.7}}$ the diameter of the *aorta*, i. e. as 1 to 37.4; and consequently the *momentum* of a single globule in such a capillary artery would be to its *momentum* in the *aorta*, as 1 to 1398.

2. FURTHER, the loss of motion from friction depends not only upon the smallness of the vessels, but also upon their distance from the heart: for, if two cylindrical pipes, whose common diameter is $\frac{3.45}{1000}$ parts of an inch, and whose lengths are 2 feet and 8 feet, be screwed into the side of a vessel full of water, at the distance of four feet from the top, the quantities discharged in half a minute will be $97\frac{1}{2}$ ounces by the long pipe, and 175 ounces by the short one. Hence the velocities of the water in the two pipes were as $97\frac{1}{2}$ and 175: so that, by the greater quantity of friction in the longest pipe, the water lost above $\frac{2}{5}$ of its velocity *.

3. AGAIN, the velocity of the blood will be different according to the different angles at which the branches go off from their trunks; and the various flexures and convolutions of the small arterial ramifications must increase the friction in them, and consequently retard the motion of the blood considerably. This seems to be confirmed by an experiment of the excellent Dr Hales; from which it appears, that the velocity of the blood in the small arteries decreases in a greater proportion than it ought to do by the above-mentioned experiments made with straight cylindrical pipes; for, having slit up the intestines of a dog from one end to the other, on the side opposite to that where the blood-vessels enter them, and having fixed a brass tube into the descending *aorta*, he found that, with a pressure equal to the force of the heart, only $\frac{1}{3}$ of the water passed in a given time through the slit arteries of the intestines that flowed through the mesenterics when cut through at their entry into the intestinal tube; notwithstanding that the area of the orifices of all the former exceeded that of the latter, and that the diameters of

* Robinson's Animal œconomy, prop. 1. exp. 1.

of the cut mesenterics did not exceed four times the diameters of the converging slit arteries of the intestines *.

FROM what has been said it appears, that the velocity of the blood will not be the same in all the arteries of the same diameter, (as some have imagined), but will be greater or less, according to their distance from the heart, the excess of the areas of the branches above their trunks, the angles at which they go off, and the number and degree of their flexures.

AGREEABLE to this, Hales observed, that, in a capillary artery of the lungs of a frog (where the distance from the heart is but small, and where the excess of the areas of all the branches above their trunk is not near so great as in the other parts of the body), the blood moved forty-three times faster than in a capillary artery of one of the muscles of the *abdomen* †: and it is probable that, next to the lungs, the blood moves quickest through the vessels of the heart. In consequence of this quick circulation, it must be evident, whether we suppose animal heat to arise from the friction of the blood on the sides of the vessels, or from an intestine motion, that, *cæteris paribus*, more heat must be generated in the lungs and heart than any where else; and hence the necessity of continual supplies of fresh air to cool the blood in its passage through the pulmonary vessels. Nor is this opinion founded in theory alone; for, upon trial it will appear, that the greatest heat in animals is, almost always, about the heart. In a jackdaw, the heat under the wing raised the mercury in the thermometer to 104 degrees of Fahrenheit's scale; in the *intestinum rectum*, it rose to 107½; and, when applied to the heart, to 109. And agreeably to this, I have found the heat in the heart of a pigeon above a degree greater than that in the *rectum*.

UPON the whole, if the *momentum* of a single red globule of blood arising from the pressing force of the heart, does not in its capillary artery, even bating friction, exceed twice its own weight or $\frac{1}{25471350}$ part of a grain; and if that loss of motion which it must have sustained by friction in its way from the heart thither be consider-

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able,

* Hales's Statical essays, vol. 2. exp. 9.

† Ibid. vol. 2. p. 68.

able, as one may reasonably conclude from what has been advanced upon this head; it will follow, that the remaining force of such a globule, when it arrives at a red capillary artery, will probably fall short of its own weight, and will be so extremely small that it can scarcely be supposed sufficient to overcome the resistance it must meet with in passing through a vessel by which it is closely embraced on all sides, although the fluid before it in the capillary veins were no obstacle in its way.

I desire that it may be here understood, that the above calculations are not offered as demonstrations, but rather as illustrations, in the present question concerning the force of the blood in the smaller vessels; and, allowing that by them the *momentum* of a red globule in its capillary artery comes out too small, either from our having, with Dr Hales, rated the force of the left ventricle of the heart too low, or, with Keill, the number of branchings of the arteries, and the proportion they bear to their trunks, too high; yet it is evident, that the force of the heart is insufficient to push the fluids thro' all the inferior orders of vessels; or, which is the same thing, that the left ventricle of the heart does not, by its direct projectile force at every contraction, move forwards the whole circulating fluids in all the vessels of the body.

HALES observed the motion of the blood to be accelerated by every *systole* of the heart, not only in the small arteries, but also in the nascent veins of the lungs of a frog*; and Leeuwenhoek assures us he has seen the same thing in other parts of various animals: so that it is scarce to be doubted, that the projectile force of the heart reaches at least as far as the capillary arteries of the first order, nay, is probably continued for some small way along their corresponding veins, especially when they are near the heart.

BUT that the *momentum* of the blood in the red capillary arteries, at any considerable distance from the heart, must be very small, will appear from an observation of Dr Hales; according to which the velocity of the blood in one of these arteries in the *abdomen* of a frog, was near 900 times less than the equable velocity of this fluid in the *aorta* of a man†; and consequently 2.6 times less than

we

* Statical essays, vol. 2. p. 69.

† Statical essays, p. 47. and 68.

we have computed it to be in a human red capillary: wherefore the excess of the *momentum* of a red globule, in such an artery of a frog, above the resistance it had to overcome, only amounted to $\frac{1}{17334000}$ part of a grain, and so must have fallen a good deal short of $\frac{1}{3}$ of its own weight; supposing the globules of red blood in a man and a frog to be of the same magnitude, which does not seem improbable *.

IF then the remaining *momentum* of a red globule in its capillary artery, after having overcome the resistance of the anterior blood in its corresponding vein, does not amount to $\frac{1}{3}$ of its own weight; it is plain, that the serous and smaller globules which move along with the red ones must be applied, by the projectile force of the heart, to the orifices of the lateral serous arteries with an inconsiderable force, such as will be unable to push these fluids through the serous, lymphatic, and, for aught we know, thro' many more inferior orders of vessels.

To illustrate this matter further, we shall, upon the principles above, endeavour to investigate the force of the heart at the origin of the nerves.

LEEUWENHOEK says, that he discovered vessels in the cortical part of the brain, which could not admit a globule, whose diameter was $\frac{1}{28000}$ part of an inch †; and that he observed the fibres of its medullary substance to be either quadrangular or hexangular: hence he concludes, that they must be composed of smaller fibres, whose extreme minuteness made it impossible for him to discover their figure, nor does he think they can ever be seen distinctly by human eyes ‡.

DR PORTERFIELD has, indeed, from an experiment of Hook's, computed the diameter of a single nervous fibre to be $\frac{1}{21000}$ part of an inch ||: but, as the best microscopes have never been able to discover any cavities in the nerves, it is certain, that, if they are hollow tubes at all, the diameter of their cavities must be a great deal less than this, and perhaps fall short of $\frac{1}{200000}$ part of an inch;

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* Med. essays, vol. 2. art. 7. § 5.

† De cerebro, p. 35.

‡ Epist 34.

|| Edin. Med. essays, vol. 4.

for a microscope, which magnifies the diameter of an object 800 times, would, upon this supposition, make the cavities of the nerves appear equal to a point whose diameter is $\frac{1}{800}$ part of an inch, which is an object that may be discovered by a good eye. Leuwenhoek, it is true, when turned of eighty, pretended to have seen cavities in the nerves. But besides that no body has been able to confirm the observation since his death, even he himself could never shew them to others, as appears from the following passage in his 32d epistle: "Id unum in hoc negotio male me habet, quod cavitates illas nemini possum conspicuas exhibere; nam simulac illas oculis meis examinandas admoveo, ilico et minuto citius per efficcationem confidunt." Now, if the ultimate fibres of the *medulla oblongata* were so fine that he could discover nothing of their shape or figure, as he himself confesses, it is not probable that he could see the cavities of the nerves, which seem to be a production of those fibres, and at least equally subtle.

BUT, lest any should think that the motion of a fluid through such extremely small vessels as the nerves almost impossible, let him reflect on the infinite divisibility of matter, and particularly on the extraordinary ductility of gold, which may be so laid over silver as that the thickness of the gold (in which however the best microscope can discover no pore) shall not amount to $\frac{1}{1200000}$ part of an inch*; *i. e.* $\frac{1}{80}$ part of what we suppose the diameter of the cavity of a nerve may be: hence the particles of such a leaf of gold swimming in a fluid may pass more easily through the nerves, than a single globule of red blood does through its capillary artery.

FURTHER, a soap-bubble, according to Sir Isaac Newton, exhibits, before it breaks, a black spot upon its upper part, the thickness of which, by his computation, scarcely exceeds $\frac{1}{3000000}$ part of an inch. Hence we see, that a fluid composed of soap and water may be divided into parts whose diameter is fifteen times less than that which we have assigned to the nerves; and consequently that such a compound fluid may easily pass through their cavities.

LET us then suppose the diameter of the cavity of a nerve to amount

* Memoires de l'Acad. des sciences, an. 1713.

mount to $\frac{1}{200000}$ part of an inch, and the area of its transverse section will be 0.0000000000196; which multiplied into 90 (the height of a column of blood whose weight is supposed equal to the pressing force of the left ventricle of the heart) gives 0.000000000176 parts of a cubic inch of blood, or $\frac{1}{1240000}$ part of a grain; which would be equal to the *momentum* of the animal spirits at the origin of the nerves, arising from the impulsive force of the heart, if there were no loss of motion from friction, and if the area of the transverse section of the *aorta* were equal to the area of the transverse sections of all the extreme capillary vessels, in which the numerous branches and ramifications derived from the *aorta* at last terminate. But, if we consider how greatly the latter must exceed the former, and, upon Dr Keill's principles, enter into a computation of the effect which this must have upon the motion of the nervous fluid; we shall find, that its velocity will be to that of the blood in the *aorta* nearly as 1 to 20000; and consequently that the *momentum* of the nervous fluid, arising from the protrusive force of the heart, will be only equal to $\frac{1}{2140000} \times 400000000 = \frac{1}{850000000000}$ of a grain.

If we imagine a sphere to be composed of the particles of the nervous fluid, whose diameter is equal to the diameter which we have assigned to the cavity of a nerve; then, taking its specific gravity to be the same with that of water, its weight will amount to $\frac{1}{45228780325614}$ part of a grain, *i. e.* near 19 times more than the force with which it is pushed forward by the contraction of the left ventricle of the heart, even upon the supposition that it had met with no resistance from friction in its passage through the small vessels of the brain. Hence the *momentum* of a small sphere of animal spirits in a nerve is 38 times less in proportion to its weight, than the moving force of a globule of red blood in its capillary artery. And the difference of their forces will be still greater, in proportion to the resistance which each has to overcome; since the resistance to the motion of a fluid from friction must be, *cæteris paribus*, as much greater in the nerves than in the red capillary arteries, as the diameter of the latter exceeds the diameter of the former.

FURTHER,

FURTHER, since the longer any capillary is, the more will the motion of a fluid be retarded, and consequently its force be diminished in it; it is easy to see that in the nerves, whose cavities are so inconceivably small, but whose length is generally considerable, the force of the heart, which we have shewn to be very little, must be unable to overcome the friction, nay even the mutual attraction of cohesion betwixt them and their fluid, and consequently be, of itself, insufficient to propell the animal spirits to the different parts of the body. And this, even upon the supposition that the nerves were continued directly from the minute capillary arteries: but, if we consider how much the force of the blood must be broken in passing through the infinitely convoluted and amazingly fine vessels of the cortical part of the brain, together with the follicles in which these vessels are imagined, by some, to terminate, what we have been contending for will appear more evident.

LASTLY, Do not these arguments receive additional weight from those experiments which shew that the brain may be nourished, may perform its office, and afford sufficient supply of spirits for carrying on all the vital and animal functions, although the blood be driven by the heart into its vessels with much less force than usual? Thus the illustrious Baron Van Swieten informs us, that he tied both the carotid arteries of a dog without any observable harm to him; nay, that he continued twelve days healthful and lively: after which time he opened his skull, but could discover nothing preternatural in the brain*. Now, as in this animal the brain could only be supplied by the vertebral arteries which inosculate with the carotids, the velocity, and consequently the *momentum* of the blood, must, at the same time that it was considerably lessened in the ramifications of the former, have been so much diminished in those of the latter (by reason of the smallness of the branches with which they communicate, compared with the trunks of the carotids) as to shew that the secretion of the nervous fluid, and its derivation to the several parts of the body do not so much depend upon the force of the heart as
has

* Comment, in Boerh. Aphor. vol. 1. p. 266.

has been generally imagined, but must, in a great measure, be owing to some other cause.

HAVING shewn how inconsiderable the *momentum* of the fluids arising from the projectile force of the heart must be, in the inferior orders of vessels, and particularly at the origin of the nerves; we come now to take somewhat a different view of the matter, and to compare the force of the left ventricle of the heart with the obstacles it has to overcome, upon the supposition that at each *systole* it pushes forward the whole fluids in all the arteries and veins of the body.

BORELLI computed the resistance which the blood meets with in circulating thro' all the vessels, to be equal to 180000 pounds weight*: but tho' this be over-rating the matter much, yet, after all the abatements that can be reasonably made, there will remain a resistance far too great to be overcome by the force alone of the left ventricle of the heart; a force which cannot, in man, amount to above 60 pounds weight†, as far as can be gathered from the latest and best experiments which have been made on other animals, in order to determine the pressing force of their heart. Yet, inconsiderable as this force is, it is not to be regarded as that which is communicated to the blood in the *aorta*, but only as the pressure or weight sustained by the whole internal surface of the left ventricle of the heart just when it begins to contract; and the force with which the blood is impelled into the *aorta* will (since fluids press equally *undequaque*) bear no greater proportion to this, than the area of the orifice of the *aorta* does to the whole internal surface of the left ventricle of the heart; *i. e.* supposing the area of the orifice of the *aorta* = 0.5 of a square inch, and the internal surface of the left ventricle = 15 square inches‡, as 1 to 30; and therefore the force with which the blood is pushed into the *aorta* must fall short of $\frac{1}{3}$ of 60 pounds weight. Hence a resistance in the *aorta* equal to two pounds, will require a force of above 60 pounds exerted by the whole internal surface of the left ventricle of

* De motu animal. part. 2. prop. 73.

† Hales makes it only 51 pounds; Statical Essays, vol. 2. p. 40.

‡ Hales, loc. cit.

of the heart to overcome it: from which it follows, either that the resistance to the motion of the blood in the *aorta*, and all its branches and ramifications, must be less than two pounds, which I believe no body will affirm; or else that the protrusive force of the left ventricle of the heart alone, is unable to drive the blood through all these vessels, and consequently insufficient, without the assistance of some other power, to carry on the circulation.

If any on this occasion should, with Borelli, have recourse to the *vis percussiois*, we need only observe, that the force of the heart is not a percussive, but a pressing one; so that, altho' the least percussive force may be greater than any finite quiescent resistance, yet this will not hold true of a pressing force, which, in order to have any sensible effect, must be greater than the resistance which it has to overcome: to say otherwise, is to affirm that, with the pressing force of one's hand, we might move a mountain.

NOR is Dr Keill's account of this matter more satisfactory, *viz.* that the blood being once put in motion, a very small force in the heart may be sufficient to keep it always in that state; for this force must be equal to the loss of motion sustained by the blood in every-circulation, and consequently to the resistance which this fluid meets with in its passage thro' all the vessels of the body; a resistance by far too great to be balanced by the few ounces to which that ingenious author has reduced the force of the left ventricle of the heart*.

BUT that the foundation upon which Dr Keill proceeds is false, and that the heart can really communicate a new motion to the blood when the old one is in a great measure lost, and after all the fluids have been for some time almost at a stand, is evident from the recovery of people who have been seized with a *syncope*, and from the revival of the sleeping animals, which, in appearance, lie dead all the winter. Further, since the blood, when it returns to the right ventricle of the heart has scarce $\frac{1}{10}$ of the force with which it was thrown into the *aorta* †, it is plain that it acquires, every circulation, $\frac{9}{10}$ of its force in passing thro' the heart and lungs.

THUS

* Tentam. med. phys. 3. de vi cordis.

† Hales's Statical Essays, vol. 2.

THUS much being said to shew that the force of the heart is, of itself, not sufficient to carry on the circulation, we shall next briefly consider the alternate contraction of the *aorta* and its branches, which has been justly reckoned among the chief causes of the motion of the blood.

THE blood which is thrown out at every *systole* by the left ventricle of the heart, is not instantly transmitted through the capillary arteries into their corresponding veins, but the greatest part of it is accumulated in the now dilated arteries, and is, during their succeeding contraction, conveyed on through the smaller vessels. This contraction however of the arteries may, perhaps, be considered rather as a continuation of the force of the heart than as any new power impressed on or communicated to the blood; since it does not appear that the arteries contract with a greater force than that by which they were dilated. But, whatever may be the force with which the *aorta* and its branches restore themselves, we know that it is less than the systolic power of the left ventricle of the heart; because the blood is projected to a greater distance from a cut artery during its *diastole*, than in the time of its *systole*. Hence it follows, that, if the force of the heart is insufficient to account for the motion of the fluids through the inferior orders of vessels, the alternate contraction of the muscular coat of the *aorta* and its branches must be so likewise. It is, however, to be observed, that the sanguiferous arteries, whose numerous branches are dispersed every where through the body, must not only, by their alternate contraction, contribute to push forward their contained fluids, but also, by their dilatation, so compress the inferior orders of vessels, as somewhat to promote the motion of the fluids in them *. I shall only add on this head, that, as the alternate contraction of the arteries depends intirely upon their preceeding dilatation by the heart, so in the serous and inferior orders of arterial vessels, to which the projectile

* Vid. Edinburgh Medical essays, vol. 5. p. 2. edit. 3. p. 39. where this point is well illustrated by the ingenious Dr Gilchrist.

jectile force of the heart seems not to reach, there is no such alternate dilatation and contraction to be observed *.

WITH respect to gravity, which some have reckoned among the causes promoting the circulation, it is sufficient to observe, that in a horizontal position of the body, it can have no effect; and, in an erect one, it must retard the return of the blood by the *vena cava inferior*, as much as it promotes its motion downwards in the *aorta* and its branches.

THERE is scarcely any thing that sooner or more naturally strikes the mind of one who inquires into the causes of the motion of the fluids in the very minute vessels of animals, as well as vegetables, than that power of capillary tubes in attracting liquors. But although the attractive power of these tubes may assist us in accounting for the absorption of fluids by the vessels called absorbents, as we shall afterwards have occasion to shew; yet it must appear evident to those who are acquainted with the properties of these tubes, that this attraction can be of no use in promoting the circulation of the blood in the capillary arteries and veins; since these vessels are always full; or, if they were not, the fluids would be determined, by this attraction, equally backwards towards the larger arteries as onwards to the veins.

S E C T. II.

That the vibratory motion of the small vessels of animals is the principal cause promoting the circulation of their fluids.

HAVING shewn the insufficiency of the powers already mentioned to account for the circulation of the fluids in the very small vessels of animals, we shall now proceed to explain what we imagine to be the principal cause of this circulation.

ALTHOUGH,

* Leeuwenhoek, Epist. 65. p. 167.

ALTHOUGH, as has been observed above, the regular alternate pulsation of the arteries does not extend beyond the capillaries of the first order, except, perhaps, in parts very near the heart; yet we are not to consider the serous, lymphatic, and other still smaller vessels, as unactive canals not contributing to promote the circulation of their different fluids: on the contrary, it is probable, that these vessels are continually agitated with small alternate contractions, to which the circulation in them is in a great measure owing.

SEVERAL physiological writers have supposed an oscillatory motion in the small vessels of animals*; but few have said any thing satisfactory concerning its cause. Baglivi supposed that the membranous parts of the body derived their oscillations from the *dura mater*; and that the vascular system and fleshy fibres had theirs from the heart: but, as it is now certain, that the *dura mater* has no other motion than what arises from the pulsation of its own vessels or those of the brain; and as the alternate contraction of the arteries, depending upon their preceding dilatation by the blood thrown out by the heart, has no place in the serous, lymphatic, and inferior orders of vessels, the vibratory motions of these canals must be deduced from some other cause.

MANY experiments and observations shew that the muscular fibres of animals are so framed, as to be readily excited into contraction by a *stimulus*. The small vessels, therefore, which have a muscular coat, as well as the larger ones, must necessarily be agitated with alternate contractions, as often as they are acted upon by any thing capable of gently irritating them; but such are the blood and the finer fluids, which, while they pass slowly through the small vessels, stimulate their internal surface, and excite in them gentle but repeated contractions.

SOME of the greatest philosophers and physicians, of ancient as well as later times, have imagined the blood to be a very active fluid, endowed with uncommon qualities, and, as it were, the

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source

* Among others, the learned De Gorter, in his treatise *De motu vitali*, has not only admitted a vital oscillatory motion in the small vessels, but endeavours to shew, that, without this, the force of the heart would be unable to carry on the circulation, § 56. &c.

source of life *; nor do they seem to have been led into this opinion so much from any favourite theory, as from experiments and observations on living animals. But, without entering into, much less defending, the peculiar notions of those authors concerning the blood, we shall only say, that this fluid is well fitted to act as a gentle *stimulus* upon the sensible fibres of animals, whether we consider its composition, heat, or intestine motion: for, while the saline and other acrid particles in the blood render it proper for irritating the tender vessels, its heat and intestine motion keep all its parts in a perpetually vibrating state, which must increase their stimulating power †. Agreeably to this, we find, that, in many insects and some larger animals, the circulation becomes more languid as the weather grows colder, and that, in the winter, it is wholly at a stand, till, by the warmth of the returning spring, the particles of the fluids begin to be briskly agitated, and consequently the solids stimulated into contraction. Harvey has long since remarked, that the hearts of several shell-fishes are only seen to beat in warm weather ‡; and the curious observations of M. de Reaumur have shewn us, that the lives of insects may be lengthened or shortened, and made more or less active, by exposing them to different degrees of heat and cold §.

THUS much being said to shew, that the blood is well fitted to act as a *stimulus*, we shall offer some further considerations to prove, that the small vessels are, by its influence, really excited into alternate contractions.

I. WE are led to conclude this from what we observe in the larger canals and vessels of animals. Thus the several portions of the intestinal tube are sollicitated into alternate contractions by the aliment, air, and bile, stretching their coats and stimulating their internal surface: and, as we imagine an alternate motion in the

* Aristot. *Histor. animal. lib. 3. cap. 19.*; and Harvey *De generatione animal. exercitat. 51. 52. et 71.*

† See an *Essay on the vital and other involuntary motions of animals, sect. 3.*

‡ *De motu sang. cap. 17.*

§ *Histoire des insectes, tome 2. memoire 1.*

the small vessels necessary to promote the circulation of the fluids in them, so we know certainly, that it is the peristaltic motion of the intestines which is the principal cause of conveying the digested aliment to the *rectum*.

NOT only the auricles and ventricles of the heart, but the trunks of the *venæ cavæ* adjoining to the right *sinus venosus*, are continually agitated with alternate contractions *. The trunks of the *venæ cavæ* preserve this motion, in animals newly dead, some time after the pulsation of the heart has ceased; but no sooner is the blood contained in these vessels evacuated, and new supplies intercepted by ligatures, than their sides collapse, and remain without motion †: hence we are led to conclude, that the alternate contractions of these veins are, like those of the heart, owing to the blood acting upon them as a *stimulus*.

IT is generally allowed that the *systole* of the larger arteries, in which there is a remarkable pulsation, is owing, not only to the elasticity of these vessels, whereby they endeavour simply to recover themselves, but partly also to a proper muscular contraction of their tendineo-carnous coat ‡: and, as this is excited by the blood thrown into them by the heart, which, at the same time that it distends their fibres, gently irritates their internal surface, it seems reasonable to allow, that the smaller vessels, endowed at least with equal sensibility, must be excited into feeble but continually repeated contractions, by the gentle *stimulus* of their circulating fluids.

FURTHER, as there are some of the imperfect animals which have no heart, the circulation in them must be owing to the contractile power of the vessels themselves excited into action by the *stimulus* of the fluids. And that the vessels of those animals, which, in a natural state, have a heart, are endowed with a similar power, seems proved by observing that some monsters want a heart and
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* Essay on vital motions, &c. p. 35.

† Bartholin. Epist. cent. 4. p. 109. &c.

‡ The diminution of the strength of the pulse in an arm that is quite palsied, is a strong proof that the larger arteries act partly by a muscular power.

any thing analogous to it *, in whom the fluids must have circulated chiefly by the power of the vessels.

2. A variety of facts might be adduced to demonstrate an alternate contractile power in the small vessels of animals, and that this is exerted more or less according to the degree of irritation affecting them.

“ THUS, the steams of hot spirit of wine received into the eyes, not only occasion a greater flow of tears from their vessels, but, in a few seconds, produce an inflammation in them, that is, they make the globules of red blood enter the serous or lymphatic vessels of the *conjunctiva*. Now, as this additional *momentum* of the blood, whereby it is enabled to dilate these vessels, cannot proceed from the heart or larger arteries, since their force cannot be altered in the present case, it must be owing to the extraordinary alternate motion excited in the vessels of the eye by the spirit.

I presume it will not be alledged, that the vapour of the spirit raises an inflammation in the eye, by contracting its vessels so as to occasion an obstruction in them, and that this obstruction afterwards produces the inflammation, by lessening the number of vessels thro’ which the blood passes, and consequently increasing its force upon the obstructed ones : for, not to insist on what might be easily proved, that no obstruction can ever produce an inflammation except in so far as it gives rise to an unusual irritation ; the spirit of wine should, by contracting the serous and lymphatic vessels of the *conjunctiva*, enable them to sustain this additional force.

BUT further, why does tepid milk and water, or a poultice of bread and milk, lessen an inflammation of the eye, while acrid, astringent, and spirituous applications increase it ? According to the doctrine of inflammation, from mere obstruction, together with an increased force of the heart and larger arteries, one would imagine that the former should, by relaxing the small vessels, expose them to greater dilatations by the increased force of the blood, and thereby increase the inflammation ; while the latter should, by contracting those vessels, enable them not only to resist the blood impelled by

* Vid. Van Swieten Comment. in Boerhaav. Aph. vol. 1. p. 256. ; and Histoire de l’Acad. des sciences, 1703 ; & Memoires, 1740.

by the heart, but also to expell the obstructing red globules. But the truth of the matter is, that the tepid milk and water and poultice, by relaxing the vessels, lessen or remove the irritation and sense of pain, which pain, by raising uncommon contractions in the small vessels, was the cause of the inflammation; while acrid, astringent, and spirituous applications, tho' they tend to contract the vessels, yet, by increasing their vibratory motions, greatly augment the force of the blood in them, and therefore must necessarily increase the inflammation.

THE heat, redness, and inflammation, brought on the skin by blisters and sinapisms, are not owing to any increase of the force of the heart, or of the *momentum* of the blood in the larger vessels, tho' this is often an effect of their application; but merely to the action of those irritating substances on the cutaneous vessels, whereby the motion of the fluids in them is greatly augmented.

THE sudden redness and glowing warmth of the face, called blushing, which, in women especially, accompanies a consciousness of shame, can only be satisfactorily accounted for from an increased motion of the small vessels of the face*.

THE extraordinary flow of *saliva* which people when hungry have upon the sight or even the remembrance of grateful food, and the profuse secretion of urine which hysterical women are frequently subject to, cannot be well explained without having recourse to an increased motion suddenly excited in the small vessels of the salivary glands and kidneys; and clearly shew that the quantity of *saliva* and urine secreted by those organs, does not so much depend upon the force with which the blood is driven into their vessels by the heart, as upon the greater or lesser vibratory motions of the secretory vessels themselves. And in the same manner, is it not reasonable to believe, that the motion of the fluids in the smallest vessels every where thro' the body, is as much, perhaps more, owing to their gentle alternate contractions, than to the force of the heart and larger arteries?

THE secretion of tears, which is very little affected by the different forces with which the blood is impelled by the heart, is immediately

* See an Essay on the vital and other involuntary motions of animals, p. 115. and 116.

mediately increased in a very great degree by acrid applications to the eyes, or by certain passions of the mind.

IN the first case, the greater secretion is owing to the acrid matter, which, by its irritation, raises an uncommon alternate motion in the lachrymal vessels. Nor can it be well objected here, that acrid substances applied to the eyes or received into the mouth, occasion a greater flow of tears or *saliva*, not by raising any stronger motion in the small vessels of the lachrymal and salivary glands, but merely by contracting their excretory ducts, and so squeezing out the liquors contained in them; since the quantity of tears and *saliva* discharged in such cases shews, that not only the excretion but the secretion in these glands is greatly increased. And if an irritation of the *pelvis* of the kidney, or *ureter*, from a stone lodged there, often occasions an uneasy sensation in the extremity of the *urethra* *; is it not reasonable to think, that, upon the application of stimulating things to the orifices of the lachrymal and salivary ducts, these will not be affected alone, but the irritation will, in some degree, be communicated to the small secretory vessels of their respective glands, so as to excite in them stronger and more frequently repeated contractions, and consequently increase their secretions?

THE flow of tears which accompanies certain affections of the mind, is, like the greater secretion of *saliva* from the sight of grateful food, and the heat and redness of the face from a consciousness of shame, owing to an unusual vibratory motion excited in the lachrymal vessels in consequence of these affections, and not to any compression which the lachrymal gland may suffer from some of the neighbouring muscles, which are then brought into contraction; for no degree of alternate compression applied to this gland remarkably increases the secretion of tears, unless its vessels, or those of the eye, are thereby irritated.

3. WE have already seen, that an increased alternate motion in the small vessels occasions a quicker flow of liquors through them: and the following case will shew, that, when this motion is much diminished

* Van Swieten Com. in Boerh. Aphor. vol. I. p. 301. ; et Morton de phthisi, lib. 2. c. 3.

diminished or wholly suspended, these vessels collapse, and the circulation in them either becomes languid, or ceases.

A boy betwixt four and five years of age was, on Saturday afternoon, suddenly seized with an apoplexy or abolition of sense and voluntary motion. On Sunday morning, at nine o'clock, when I first saw him, his pulse was full and quick, and his eyes had something of a glazed appearance; but in the evening this was more remarkable. Monday, a little before noon, he was still alive, but his breathing was very laborious, and his pulse small and quick; at this time, his eyes were more shrivelled than they used to be in those who have been several hours dead.

THIS glazed appearance of the eyes could not be owing to the diminution of the force of the heart, since the pulse was full and strong for twenty-four hours after the disease came on: nor can the failure of the pulse, afterwards, account for the eyes appearing more shrivelled than is usual in persons newly dead. But if the circulation of the fluids in the small vessels be chiefly owing to a vibratory motion in them, and if this must cease when the influence of the nerves is intercepted; in this boy, whose brain, especially its anterior part, was so much obstructed, the motion of the fluids in the very small vessels of the *cornea* and the secretion of the aqueous humour must have been considerably diminished; and hence the dimness and shrivelling of the eyes *.

THE withering of a member that is palsied, or deprived of the nervous power, is to be accounted for in the same manner, and is a proof that the circulation of the fluids through the inferior orders of vessels, is not more owing to the force of the heart, than to the action of these vessels themselves. This withering has made some imagine, that nutrition is performed by the nerves: but the appearance, we see, is easily accounted for without this supposition; and there are good reasons to think that the nerves are solely subservient to motion and sensation.

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4. ALTHO'

* Nuck observed the secretion by the glands to be much diminished, or entirely stopt, after their nerves were obstructed or compressed. Vid. *Adenograph. curios.* p. 16.

4. ALTHO' the alternate contractions of the smaller vessels, which we have been contending for, cannot be discerned in most animals; yet they may be seen in the legs of a bug: in the small vessels of which an extraordinary vibration is discovered by the microscope *.

5. LASTLY, The vibratory, though invisible motion of the small vessels, is more confirmed, by that irregular motion, backwards and forwards, of the globules of blood in the capillary arteries, formerly observed in dying animals by Leeuwenhoek, and lately described more accurately by the learned M. de Haller †. As this motion continues after the heart is cut out of the body, it cannot be owing to any impulse of the blood communicated from the larger arteries: and we have no reason to ascribe it to any peculiar attraction between the globules of blood ‡, since this fluid, neither when received into small glass tubes, nor in any other experiment, shews such a power.

THIS oscillation, however, of the globules of blood may be accounted for from an irregular vibratory motion in the sides of the small vessels. We see that in animals newly dead, the *vena cava* is excited into alternate contractions by the *stimulus* of the blood contained in it, and that the fibres of the muscles, upon being exposed to the air, or to the action of other *stimuli*, are frequently agitated with a weak, irregular, and tremulous motion; it seems therefore reasonable to conclude, that the small arteries, which are of a similar nature with the *vena cava*, and whose power of motion in living animals shews them to be in some degree muscular, may, after the circulation ceases, by the *stimulus* of the cold air, or of the globules of blood contained in them, continue to be thrown into small but irregular contractions, which, though not observable even by the microscope, yet are discovered by their effects: for it is easy to see, that by the smallest contraction of the sides of a capillary artery, the contained blood will be put in motion, which will be renewed as often as such contractions happen.

THIS is still farther confirmed from its having been observed, that

* Baker on the microscope, p. 130.

† Aët. Cottingenf. vol. 4. p. 351.

‡ Ibid. p. 354.

that a few globules of blood extravasated between the *lamine* of the mesentery ascended and descended irregularly, and were agitated with the same kind of oscillatory motion as in the small arteries *: for as often as by the least agitation in the air, or other cause, the *lamine* of the mesentery approached nearer to one another, the globules would fly from that place; and would return to it again as soon as they receded from each other, in much the same manner as water suspended between two panes of glass is observed to ascend or descend, just as these panes are brought nearer to, or removed farther from each other.

THE objection against the reality of a vibratory motion in the small vessels of animals, because the microscope shews it not in most animals, is of no great weight; since it can scarce be doubted, that the particles of all bodies, especially fluids, are affected by heat with a perpetual oscillatory motion; and yet, unless the heat be great, the eye, even assisted by the best microscopes, cannot discern this motion.

FURTHER, since the microscope only shews the circulation of the fluids in the red capillary arteries, but not in the serous, lymphatic, and many inferior orders of vessels, can it be expected that any alternate vibratory motion should be discovered in these vessels? Or, is it reasonable to deny an alternate motion to all vessels or particles of matter which are too small to fall under the notice of our senses?

ALTHO' the branches of the vine were transparent, so that the motion of the sap in its vessels could be seen by the help of a good microscope; yet it is probable that we should not be able to discover any vibratory motion in them: and yet the force of the sap in the bleeding season shews, that, besides attraction, there must be a real propelling power exercised by the vessels of the vine †.

IF the diameter of the *aorta* in its *diastole* does not exceed its diameter when contracted above $\frac{1}{5}$ of a line, *i. e.* $\frac{1}{50}$ of its diameter ‡; and if the change of diameter, which happens in the red capillary

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arteries

* Vid. Aët. Gotting. vol. 4. p. 354.

† Vid. Hales's Statical essays, vol. 1.

‡ Vid. Weitbrecht in Comment. Academ. Petropolitan. vol. 7. p. 314.

arteries and inferior orders of vessels from their vibratory contractions, be three times less in proportion to the magnitude of these vessels than the difference of diameter in the *aorta*, arising from its alternate *diastole* and *systole*; then the difference between the greatest and least diameter of a capillary artery capable of receiving only one globule of red blood, when most dilated or contracted, will be equal to $\frac{1}{150}$ part of its diameter; *i. e.* supposing its diameter $\frac{1}{2000}$ of an inch, $= \frac{1}{300000}$ of an inch; and the space described by each side of such an artery, when it performs one of its small vibratory contractions, will be only equal $\frac{1}{600000}$ part of an inch, which is greatly too small to be discerned by the best microscope.

HAVING thus endeavoured, by a variety of arguments, to shew, that the small vessels of animals are, through the gentle *stimulus* of the fluids, continually agitated with alternate contractions, we shall now, briefly, point out their use in carrying on the circulation. And it must appear evident to every one, that the inferior orders of vessels will not only not retard the motion of the fluids, but greatly promote it; since every small portion of them will, like a little heart, by its alternate contractions, push on its contained fluid. Nor ought these contractions, however weak and imperceptible, to be judged unable to produce this effect; since the motion of the fluids in the very small vessels is far from being rapid, and just such as might be expected to arise from this cause. The Reverend Dr Hales has observed, that, in a capillary red artery in one of the muscles of the *abdomen* of a frog, the blood moved only an inch in a minute and a half *: and it is probable, that, in the finest

* Statical essays, vol. 2. p. 68. Leeuwenhoek and the illustrious M. de Senac have also observed the motion of the fluids to be very slow in the small vessels. On the other hand, M. de Haller, in his late treatise on the motion of the blood, tells us, that he has frequently seen this fluid moving faster in a small arterial branch in the mesentery of a frog, than in the trunk whence it took its rise (a). But we cannot conclude from this, that the velocity of the blood is greater, in a natural state, in the small branches than in their trunks; for this is repugnant to the most certain observations, which shew, that the capacity of the branches always exceeds that of the trunks from which they proceed. All therefore that can be fairly deduced from M. de Haller's observations is, that the blood may move as fast or even faster in some of the small

(a) Act Gotting. vol. 4. p. 294. 295. and 299.

finest secretory vessels of the brain, the fluids may not move above $\frac{1}{12}$ of an inch in a minute, *i. e.* not twice as fast as the minute-hand of a small-sized watch.

If it be objected, That, as the capillary arteries and veins are without valves, their alternate contractions must push the fluids equally back towards the heart, as onwards to the larger veins: it may be sufficient to answer, That the resistance arising from the semilunar valves of the *aorta*, and from the force of the heart and larger arteries *a tergo*, being greater than that which opposes the conveyance of the fluids into the larger veins, the fluids acted upon by the small vibrating vessels must necessarily be determined towards the latter. But further, why may not the alternate contractions of the small vessels, like the peristaltic motion of the intestines, proceed in such manner as to impel their fluids more sensible onwards to the veins than backwards to the larger arteries?

UPON the whole, as we conceive the motion of the blood in the larger vessels, and even capillaries of the first order, to be owing to the alternate *systole* of the heart and arteries; so in the serous, lymphatic, and still smaller vessels, where this force either reaches not at all, or is greatly diminished, the circulation seems to be carried on chiefly by the vibratory motions of those vessels themselves: and, the finer fluids being in this manner conveyed into the larger veins, the pulsation of the neighbouring arteries, the action of the voluntary muscles, and the alternate compression made upon all the contents of the *abdomen* and *thorax* by the motion of respiration, will promote their return to the heart along with the red blood in the *venæ cavæ*.

WHAT we have said of the circulation of the fluids in general, we would have understood also of their motion in the secretory ducts of the several glands. In those glands whose vessels are most patulous, the secretion may be partly, and indeed principally, carried on by the force of the heart and larger arteries; a proof of which seems

small arterial branches than in the trunks from which they take their rise, as often as those branches are affected with an unusual irritation, or the other branches proceeding from the same trunk are obstructed or contracted by cold or other causes.

seems to be the bloody urine voided by such as have weak kidneys, after violent exercise: but in other glands, whose structure is finer, and particularly in the brain and *testes*, the motion of the fluids in the secretory and excretory vessels seems to be much less owing to the force of the arterial blood *a tergo*, than to the gentle vibratory contractions of the vessels themselves.

WITH regard to the nerves, which are generally considered as the excretory ducts of the brain; it is probable, that the conveyance of their fluid to the various parts of the body is not only owing to a gentle oscillation in them and their surrounding membranes, but also, in some degree, to their attraction as capillary tubes; for no sooner can there be a waste of this fluid at the extremity of any nerve (whether this happens from exhalation, alternate compression of the neighbouring parts, or any other cause) than, by its attractive power, it will be filled again. In the other glands, however, whose excretory ducts, by their union, soon form pretty large canals, no such attraction will have place.

1. FROM what has been said, it may appear, that we are not to consider the force of the heart and contraction of the larger arteries, as the sole causes of the circulation of the fluids in animals. The whole vascular system is endowed with a moving power, which is constantly excited into action by the *stimulus* of the circulating fluids; so that while the small vessels, by means of friction, destroy in part the *momentum* of the juices, they, at the same time, communicate, by their gentle vibratory contractions, a new impulse to them. Every part therefore of the vascular system, as well as the heart and larger arteries, nay every section even of the smallest vessel, is to be conceived as promoting the circulation of the fluids; that great work, upon which the life of the whole depends, and in the carrying on of which almost every part of the body is active.

2. IF the motion of the fluids in the inferior orders of vessels be not so much owing to the force of the heart and larger arteries as to the alternate contractions of those vessels themselves, we may easily see why frictions, warm, penetrating, and stimulating fomentations,

mentations, and cataplasms, &c. are often more successful than internal medicines, in removing obstructions in the serous, lymphatic, and other small vessels; since they not only tend to attenuate the obstructing matter, but increase the oscillatory motion of those vessels. For the same reason, warm mineral waters, pumped with considerable force upon a part affected with the rheumatism or *sciatica*, have effected a cure after other remedies had been used in vain.

WARM spirit of wine, either alone or mixed with other things, proves often a good deobstruent when applied externally: yet I have known some people who were afraid to use it with this intention, lest it should coagulate the *serum* of the blood: but their fears were groundless; for the quantity of spirit of wine which enters by the pores of the skin, is too small to coagulate; besides, as it is taken up by the absorbent veins, it must go to the heart, and be mixed with the mass of blood, before it can come at the obstructed vessels, unless when the obstruction happens to be in any of those glands in which the lymphatics terminate; for since my very ingenious Colleague Dr Monro *junior* has proved these lymphatics to be no more than absorbent veins*, they must carry the finer parts of such substances as are applied to their mouths directly to those glands which they enter, and before they can be mixed with the mass of blood. But, although little is to be expected from the solvent quality, or to be apprehended from the coagulating power of the spirit of wine; yet in many cases it proves a good deobstruent, by raising a more than common oscillatory motion and heat in the vessels of the part to which it is applied.

3. If the circulation in the small vessels be, in a great measure, owing to their vibratory motion excited by the *stimulus* of the circulating fluids, it will follow, that, when these vessels, in any part of the body, are affected with an extraordinary irritation, they must be agitated with much stronger and more frequently repeated contractions than usual: whence the force of the blood in them will be much increased; and the small arteries will not only be, more
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* Vid. Dissert. de venis lymphaticis valvulosis.

than usually, distended with blood, and consequently the part inflated, but the red globules will be pushed in to the serous vessels *, and in many cases will be forced into the spaces of the *tela cellulosa*, through the dilated orifices of the small arteries, which terminate in them †; and this must happen whether the force of the blood be, or be not increased in the other vessels of the body. An inflammation, therefore, is not owing to an increased force of the heart and larger arteries consequent upon an obstruction, as some authors of great name have imagined, but to an increased alternate contraction in the small vessels, whether this arises from some obstructing matter overstretching their fibres, or acrid matter irritating them. An obstruction without an irritation in the obstructed part, never occasions inflammation; but the irritation of any sensible part with a sharp instrument, or acrid matter, never fails to produce this effect, although there be no preceeding obstruction, nor increase of the force of the heart. When a large artery is tied in the operation of the aneurism, we do not find, that the increased *momentum* of the blood in the neighbouring arteries produces an inflammation in the arm; but, when a tendon is wounded in bleeding, or a little acrid matter is collected below the nail, a remarkable pain, swelling, and inflammation ensue.

HOWEVER, although an increased force of the blood in the large vessels is not the cause of inflammation, yet it is frequently the consequence of it: for, as often as the inflammation is great, or the part inflamed very sensible, the whole nervous system will be so affected by the pain, as to render the heart and larger arteries more irritable, whilst the blood, now vitiated by the obstruction and inflammation, must act upon them as a stronger *stimulus* than usual.

Hence

* Although an inflammation *ab errore loci* may not happen so often as has been alledged, yet the inflammation of the *cornea* and *conjunctiva* covering it, whose vessels in a natural state do not admit red globules, is a sufficient proof that inflammations have not only their seat in the red capillaries, but also in the serous arteries. Nay, the effusion of blood into the spaces of the *tela cellulosa*, is itself a proof of an *error loci* in inflammations, since this effusion is much seldomer owing to a rupture of the small red arteries, than to a dilatation of the orifices of those vessels which in a natural state only transmit a thin colourless fluid.

† Haller. Element. physiolog. tom. 1. lib. 2. sect. 30.

Hence we may see, why, in inflammations, the pulse is often little changed till the disease has continued for some considerable time. In inflammations of the stomach, intestines, and *uterus*, the pulse, though much quickened, often continues small; because, on account of the great sympathy between the nerves of these organs and those of the heart, the heart becomes so irritable as to contract before its ventricles are filled with the returning venous blood.

FROM what has been said it may appear, that, in the cure of inflammations, besides diminishing the force of the circulation in general by bleeding, a particular regard is to be had to the vessels of the part affected, whose extraordinary alternate contractions should be lessened by proper emollient and anodyne applications, and, in many cases, by blistering the neighbouring parts. Dr Pringle has often observed the good effects of blisters, even when early applied in pleurifies and other internal inflammations *. And I have frequently seen a blister, in fifteen hours, remarkably lessen the quickness of the pulse, not only in obstructions of the lungs attended with a fever and considerable expectoration of phlegm †, but also in pleuritic cases, and in an *angina*, after bleeding once and again had had little effect; nay, in obstructions of the lungs and pleurifies, I look on it as one of the worst signs, when after proper bleeding a large blister does not lessen the quickness of the pulse; for I have rarely seen any such cases that did not prove fatal. Many physicians are prejudiced against blisters in inflammations, because, by their irritation, they are supposed to increase the force of the circulation in general: but, not to mention, besides, the good effects which they may have by attenuating the obstructing matter, and making a derivation of ferous humours from vessels which are nearly connected with those of the part affected; if the account we have given of inflammation be true, it must follow, that although the material cause of an inflammation, *i. e.* the acrid or obstructing matter, be not immediately removed by blistering; yet if, according to Hip-

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pocrates

* See his Observations on the diseases of the army, 1st edit. p. 173. 178. and 179.

† See Philosophical transact. vol. 50. part 2. p. 569.

pocrates *, the painful sensation in the inflamed vessels be lessened by its means, the extraordinary alternate motions of these vessels, and consequently the cause which continues and increases the inflammation, must also be lessened. Hence it appears, that a blister, though it tends to increase the force of the circulation in general, may yet lessen the *impetus* of the blood upon the vessels of an inflamed part even more than bleeding. In patients in whom there is no fever or increase of the circulation from any inflammation, blisters are observed, by their *stimulus*, to raise the pulse and augment the heat of the body; but in internal inflammations, after such bleeding as the circumstances of the patient may require, blisters often abate the fever and heat of the body, as well as the quickness of the pulse, by lessening or removing the inflammatory obstruction.

WHAT has been said of blistering, may be applied to cupping and scarifying in pleurifies, *angina's*, &c.

SINAPISMS, laid to the soles of the feet, remove or lessen a *delirium*, not by determining the blood more copiously to the inferior extremities, for their effect in this respect is trifling; but by raising a considerable pain, which so affects the mind, as to render it less sensible of the unusual *stimulus* or irritation in the brain, or its membranes, *i. e.* of the cause producing and continuing the *delirium*. Nor is it material to what part of the body those cataplasms are applied; for a strong *delirium*, in a fever, has been removed by the application of a sinapism, by mistake, instead of a poultice of *theriaca*, to the region of the stomach.

WE may also, from what has been said, see how ravings, phrenies, and madness have been cured by the power of music †, or by a sudden fright ‡; for these, by greatly affecting the mind, and fixing its attention, not only render it less sensible of the disordered state of the brain and its membranes, but, by the strong impression they make on the *sensorium commune*, may tend to dislodge or remove the cause of the disease.

S E C T.

* Δυσ πόνων ἅμα γινόμενων, μή κατὰ τὸ αὐτὸν τόπον, ὁ σφοδρωτέρως αἰσχυρὸς ἢ ἕτερον. Duobus doloribus simul obortis, non in eodem loco vehementior obscurat alterum. Aphor. lib. 2. No. 46.

† Histoire de l'Acad. des sciences, 1708 & 1717.

‡ Van Swieten Comment. in Boerhaave Aph. § 11.

S E C T. III.

Of the motion of the fluids in those vessels of animals commonly called absorbent.

BESIDES the small veins, which are continued vessels with the arteries, and terminate at last in the two *venæ cavæ*, there are others which take their rise from the internal surfaces of the several cavities in the body, and from the skin: and, as the fluids which these vessels convey cannot be impelled into them by the force of the heart or arteries, they have been thought to receive them by suction, and therefore have been called absorbent or imbibing veins. In the intestines we find two kinds of them, *viz.* the lacteals, and those commonly called absorbent veins; which last are also to be found upon the surface of the skin, *peritonæum*, *pericardium*, *pleura*, vesicles of the lungs, *dura* and *pia mater*, and, in short, upon every membrane which lines a cavity. In accounting for the motion of the fluids in these vessels, we shall begin with the lacteals; in order to which it may be necessary to premise,

1. THAT the lacteal veins have their origin in the villous coat of the intestines where their orifices are so small as to escape the eyes of anatomists: that leaving the posterior surface of the villous coat, they pass through the nervous and muscular coats, and, uniting into larger canals, are distributed in the form of a network in the external cellular membrane of the intestines; and that after this, they enter the mesentery, and are furnished with numerous valves, which hinder the return of the chyle to the intestines.

2. As often as the muscular coat of the intestines is contracted, the lacteal veins, which pass between the interstices of its fibres, and are distributed in the nervous and external cellular membranes, must be compressed; but are relaxed and freed from this pressure, when this coat ceases to contract.

3. IT is well known that small glass tubes are endowed with a power of attracting fluids, so as to raise them considerably above the liquors in which they are immersed. That this power increases exactly in the inverse *ratio* of their diameters. That these tubes, whether straight or crooked, in a perpendicular or oblique position, in *vacuo* or in the open air, attract fluids to the same height, provided their diameters be equal. That, when a capillary glass tube ends in a larger canal, the fluid is elevated so as to fill the capillary part, but does not ascend any further. That, if the diameter of a glass tube exceeds $\frac{1}{10}$ of an inch, its power of attraction is scarcely perceptible: and lastly, That the same glass tubes attract different fluids to different heights, and this neither in proportion to the tenacity nor gravity of the liquors. From all which it is natural to conclude, that the lacteal veins, which, in their beginning at least, are smaller than any glass tubes hitherto made, must be endowed with a considerable power of attracting the chyle, when it is applied to their orifices.

How far the attractive power in such canals as the lacteals and other absorbent veins is, *ceteris paribus*, greater or less than in glass tubes, we have no experiments to determine: but as the urine, an animal liquor, is more strongly attracted by glass capillaries than water or any other fluid *; it is not unreasonable to suppose that animal capillaries may have a still stronger power of attracting it. And, as the same fluid is differently attracted by capillary tubes of different substances, though of the same diameter †; is it not probable, that the several absorbent veins in animals may be so formed as to attract their proper liquors most strongly?

FURTHER, the remarkable attractive power of which the small vessels of vegetables are possessed, and by means of which they draw from the same earth very different juices, is no small argument for allowing a similar attraction in the vessels of animals. It is by this power that the sap continues to rise in the vessels of trees, even in the winter, though slowly and in a small quantity; nor can it be alledged, that

* Muschenbroeck de tub. capill. vitr. cap. 3.

† Muschenbroeck Element. philos. natural. cap. 18. § 531.

that it is the heat of the sun which promotes the ascent of the sap then, as it does in summer; since trees in cold cloudy weather, provided it be dry, and in places which the sun in winter cannot reach, take up continually, by their roots, as much moisture as is necessary to supply the waste by perspiration in their trunks and branches. And Dr Hales has observed, that cut branches will imbibe from the small end immersed in water to the great end, as well as from the great end immersed in water to the small end *; hence it follows, that the ascent of the sap in the vessels of plants, is not owing to any peculiar structure, but solely to capillary attraction.

It is true indeed, that though capillary attraction makes the sap to rise in plants, it cannot, without the assistance of some other cause, make a continued flow of it from the roots to the branches and leaves; because, as soon as capillary tubes are filled, or have raised fluids to a certain height, all motion from attraction ceases: but as the action of the air and the sun upon the trunks, branches, and leaves of plants occasions a plentiful perspiration of the sap by their pores, a proportional quantity will be attracted from the earth by their roots to supply this waste, and to keep the capillary vessels full. However, as often as the absence of the sun and the moist state of the air put a stop to the perspiration of vegetables, the sap ceases to ascend: nay, if the earth be warm and dry, the sap acquires a retrograde motion: and hence it is, that, in a cool summer's evening when the dew begins to fall, vegetables attract the watery particles in the air by the pores of their leaves and branches, in like manner as they had done the moisture of the earth by their roots in the day-time †.

THESE things being premised, it will be easy to account for the passage of the chyle into the lacteal veins.

WHEN any proportion of the intestines is relaxed, the lacteal vessels, which open every where on the surface of the villous coat, receive the chyle by their attractive power, so as to fill those branches which are dispersed in the membranes of the gut. The chyle being thus admitted into the capillary lacteals, is by the succeeding contraction

* Statical Essays, vol. 1.

† Vid. Hales's Statical essays, vol. 1.

traction of the muscular coat of the intestine, which compresses them, pushed on towards the mesentery. As soon as this contraction ceases, the emptied lacteals, being free from compression, fill themselves with chyle as before, which the next contraction of the gut presses forward to the larger lacteals in the mesentery. And thus the chyle is by turns attracted and propelled by the capillary form of the lacteals and the peristaltic motion of the intestines.

FURTHER, it is probable, that the lacteal veins, like the other small vessels of animals, have a vibratory motion excited by the gentle irritation of the chyle, which assists the alternate contractions of the intestines in the propulsion of that fluid. Without supposing such a motion in the umbilical veins of the chick, it will be hard to account for its growth during the time of incubation. It is true, the umbilical arteries and veins run close together in oviparous as well as viviparous animals, so that the alternate pulsations of the former must contribute to the propulsion of the fluids in the latter towards the heart. But, as there is no pulsation to be observed in the heart or umbilical arteries of the chick, till towards the end of the second day *; and as, at any rate, the pulsation of the umbilical arteries does not extend beyond the capillaries containing red blood, the fluids in the extreme branches of the umbilical vein must owe their motion to some other cause. And is it not reasonable to believe that the colliquated white is conveyed thro' these vessels by their attractive power, as capillary tubes, assisted by the small alternate contractions excited in them by the gentle *stimulus* of this warm fluid? And we are confirmed in this opinion by analogy from plants, in whose vessels the circulation of the sap is assisted by a vibratory motion, which seems to be excited in them chiefly by the heat of the sun. And is not the extraordinary force of the sap in the bleeding vine owing to its vessels being susceptible of stronger vibrations than those of most other plants †?

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* Malpigh. de ovo incubato.

† Dr Hales has observed, that, in a stem of a vine $\frac{7}{8}$ of an inch diameter, the force of the sap in the bleeding season was five times greater than the force of the blood in the crural artery of a horse. Statical Essays, vol. 1. exp. 36.

THE chyle in the larger lacteals which run along the mesentery, and have valves, is pushed on to Pecquet's receptacle by the new chyle continually transmitted to them from the intestines, by the pulsation of the sanguiferous arteries which run contiguous with them, and by the alternate motion of the diaphragm and abdominal muscles in respiration.

IF the chyle is received into the nascent lacteal veins of the intestines by their attraction as capillary tubes, it will be easy to see why quick-silver, which is repelled by such tubes, should, when swallowed by itself, generally pass through the intestines without almost any of it getting into the blood. On the other hand, if the propulsion of the chyle is owing to the alternate contractions of the intestines, it will appear, why it should cease to be transmitted thro' them soon after death; and why, in a well-fed animal newly killed, the lacteals in the mesentery, after being emptied, may be filled again, by gently pressing the intestines, and thereby imitating their peristaltic motion.

WITH respect to the absorbent veins of the intestines; the finer parts of the digested aliment received into them by their attraction, are propelled towards the larger meseraic veins and *vena portarum*, by the alternate contractions of the muscular coat of the intestines and pressure of the abdominal muscles and diaphragm in respiration. But, as these absorbents are not provided with valves, like the lacteals, it may be inquired, Why the last-mentioned power does not press the absorbed fluids equally backward to the intestines, as forward to the *vena portarum*? This we imagine is prevented,

1. BY the gentle alternate contractions of the absorbent veins, which contractions, as they are owing to the *stimulus* of the imbibed liquor, must begin at their orifices, and proceed towards their larger trunks. Such a motion as this, tho' small, will determine the course of the fluids on to the larger veins, but oppose their return to the intestines. And we find in fact, that, by means of a similar motion in the intestines, the useless part of the aliment is conveyed to the great guts, even in a horizontal position of the body, where
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the alternate preffure of the diaphragm and abdominal mufcles ought to push the contents of the bowels as much backwards to the ftomach as forwards to the *colon*.

2. BUT when any portion of the inteftines is contracted, the naſcent abſorbent veins, which riſe from the villous coat, and paſs thro' between the other membranes of this part, muſt have their fides preſſed together, ſo as to allow nothing to paſs through them; wherefore the preſſing force of the mufcles of reſpiration muſt (if acting at this time upon the larger trunks of the abſorbent veins), propell their fluids towards the *vena portarum*. When this portion of the inteſtine is relaxed, the emptied abſorbents, by their attraction, will fill themſelves with new fluids from its cavity: ſo that, whether the inteſtines are contracted or relaxed, there will be always ſome obſtacle to the retrograde motion of a fluid in the abſorbent veins.

WHEN the liquors taken up by the capillary abſorbents are conveyed into the larger meſeraic veins, they will be carried along with their blood to the *vena portarum*.

As there are, upon the internal ſurfaces of all the cavities of the body, exhaling arteries which perpetually throw out a fine fluid to moiſten and lubricate the parts; ſo there are bibulous veins which take it up; their exiſtence is aſcertained, not only by obſerving that no liquor in health is collected in thoſe cavities, but alſo by anatomical injections *.

THESE abſorbent veins, which, like thoſe of the guts, have no valves, receive, by their attraction as capillary tubes, the dewy vapour of the arteries, which they convey either to the lymphatics †, or to the ſanguiferous veins, by their vibrating motion, the pulſation of the contiguous arteries, and the alternate compreſſion of the mufcles. The abſorption in the cavities of the *abdomen* and *thorax* is promoted by the alternate preſſure of the mufcles concerned in reſpiration; while the mufcles of voluntary motion employed in all kinds

* See *Kaui perſpiratio Hippocrati diſta*.

† Vid. *Al. Monro jun. Diſſertat. de ven. lymphat. valvuloſ.*

kinds of exercise and labour, by accelerating the motion of the fluids in the absorbent vessels of the trunk and extremities of the body, enable them to imbibe more copiously. And hence we may see, why animals which move little, are generally loaded with fat; while those which are kept at hard labour are very lean. In the former, the absorbent veins of the fatty cells imbibe the oily matter slowly, because they want the alternate pressure of the muscles of voluntary motion to push their contained fluid forward to the larger veins: In the latter, the absorption from those cells is not only increased by the various and continually repeated pressure of the acting muscles, but the body being, by much exercise, in some measure exhausted of fluids, the veins imbibe more greedily, while the exhaling arteries pour forth their oily liquor more sparingly.

IF the exhalent vessels of any cavity throw out too much, or if the absorbent power of the veins be weakened, or if both these happen together, a watery fluid will be collected in it; and in this way are produced an *ascites*, *hydrocele*, *hydrops pectoris*, &c.

WHEN the blood is thin and watery and the vessels weak, anasarca, œdematous, and other dropical swellings are common: for, as the bibulous veins, by their attraction, can only take up fluids in proportion to the depletion they suffer by means of their own vibratory contractions, and the alternate compression of contiguous arteries and muscles, their absorbing power must necessarily be lessened under a lax state of the fibres.

FURTHER, while the redundance of a watery fluid in the blood increases the exhalation by the small arteries, it lessens the absorption by the veins, for the same reason that ashes, sugar, or salts, when moistened, attract the watery particles of the air less than when they are dry.

AGAIN, although there be little or no fault in the blood itself, yet, if its return from any part to the heart be much retarded, a dropy of that part will soon ensue, because the fluids taken up by the absorbents will be slowly and not without difficulty received into the larger sanguiferous veins: and, as we have just now observed, their absorption must be in proportion to their depletion.

Hence we see why scirrhus tumours, ligatures, and whatever compresses the veins, soon bring on dropical swellings.

It also appears from what has been said, in what manner diuretics and cathartics carry off the stagnating waters in an *ascites* and other dropies: for by the discharges they make by the kidneys and intestines, they not only lessen the quantity of watery fluid in the blood, but also, by their *stimulus*, increase the force of the circulation, whence the exhalation by the arteries must be lessened, at the same time that the absorption by the veins is increased.

THE surface of the skin and vesicles of the lungs, like the other surfaces in the body, are furnished with exhaling arteries and absorbent veins: by the former a fine lymph is perpetually discharged from the blood; and by the latter, the aqueous particles floating in the air are constantly conveyed into it.

WHEN the air is moist, and the body has been exhausted by fatigue, the absorption by those veins often exceeds the exhalation by the arteries, as Drs Keill and Linning have observed*: but, taking the whole year round, the perspiration made by the skin and lungs exceeds their absorption by about forty ounces a-day in Great Britain, and fifty-four ounces in South Carolina; which, tho' it has been commonly reckoned the total of the perspiration, is really no more than its excess above the quantity of fluid taken in by the absorbent veins of the skin, *fauces*, and lungs.

ALTHOUGH in vegetables, the vessels which perspire in the heat of the day, frequently exert a contrary function in the night, and imbibe the dew and watery particles then floating in the
air;

* Medicin. Stat. Britan. tab. iv. et observat; et Philosoph. transact. No. 470.

The remarkable absorption by the skin observed by Dr Linning, July 3. 1740, betwixt 2 $\frac{1}{2}$ and 5 $\frac{1}{2}$ after noon, happened, 'tis true, without any preceeding fatigue; but is easily accounted for from his having, in that time, voided 28 $\frac{1}{2}$ ounces of urine; since so great a waste of the thinner parts of the blood must not only have diminished the exhalation by the cutaneous perspiring arteries, but also have increased the absorbent power of the imbibing veins every where through the body: and hence it is, that in a *diabetes* the urine often not only exceeds the quantity of liquors drank, but these are taken up so fast by the absorbent vessels of the stomach and intestines, as to be discharged by the kidneys, before one would imagine that they had time to get into the blood.

air; yet it is not probable, that the exhaling or perspiring vessels of animals thus become imbibing ones, or that the moisture of the air can by them be conveyed into the blood: since any motion in these vessels, from their extremities to their larger trunks, must be in opposition to the course of the arterial fluids.

THE absorption by the vessels of the skin is performed in the same manner as in the other absorbents; only it is probable, that the oscillations of the external air perpetually varying may concur in promoting it.

ALTHO' the exhalations from animal, vegetable, and mineral bodies, may, along with the aqueous particles in the air, be taken into the blood by the absorbent veins of the skin and lungs, (and thereby account for pestilential and epidemical diseases raging at particular seasons), yet it is not probable, that elastic air can be imbibed by these vessels, and so conveyed into the blood: for it has been observed, that air moves with great difficulty through capillary glass tubes, tho' some hundred times larger than the pores of the skin*: and it is well known, that water and other fluids can penetrate many substances thro' which air cannot pass.

THIS observation of the difficulty with which air moves through capillary tubes, may serve to decide an old controversy amongst physiologists, viz. Whether or not any elastic air enters into the blood by the lungs? For, since a few drops of water, with small portions of air between them, in a capillary tube, require a greater force to make them ascend than that with which the tube attracts the particles of the water†, it will follow, that, if any elastic air were admitted into the absorbent veins of the lungs, it would not only not move, but would hinder their absorption of other fluids.

THE great swelling of animals in an exhausted receiver likewise shews, that air cannot readily pass through the small pores of the

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skin

* Aërem vero non nisi tardè et cum quadam tenacitate per hos tubos moveri, semper docuit experientia; aëri enim inest species quædam tenacitatis aut immobilitatis; Muschenbroeck de tub. capill. vitr. cap. 1. exp. 11.

† Muschenbroeck. loc. citat.

skin and lungs. Nor does it seem to be an objection to this doctrine, that air has been found in the cavities of the heart; since, in a morbid state, this air might have been separated from the blood *.

It is observable, that air injected into the veins of an animal, occasions obstructions, concretions in the blood, and sudden death; which effects, however, may be accounted for, from the power which air has of coagulating blood, and of stopping the motion of water, even in large pipes, especially in their flexures †.

BUT to return; as the effluvia of different substances floating in the air, are, by means of the cutaneous absorbents, conveyed into the blood, so likewise are the finer parts of plasters, cataplasms, fomentations, and other applications: which ought therefore to be considered, not only as having a local influence, but also a general one upon the whole body by their subtile parts, which are mixed with the blood and other fluids.

To some it may be a difficulty, that quick-silver applied in the form of an ointment, should be so readily taken up by the absorbent vessels of the skin; since it passes through the intestines without getting into the lacteals. But this happens from the particles of the mercury being minutely divided, and so united with those of the grease as to enter the pores of the skin along with them: for, though quick-silver is repelled by capillary glass tubes, yet, if their internal surface is besmeared with melted grease, it will be attracted by them ‡.

WE are informed, that, upon opening the bodies of such as had taken mercury in large quantities, this fluid has been sometimes found in the cells of the bones and elsewhere §; the reason of which may be easily explained from what has been said; for, if the very subtile and minutely divided particles of mercury (after they are

* Hales's Statical Essays, vol. 1. chap. 6.

† Philosoph. Transact. No. 393.

‡ Memoires de l'Academ. des sciences, an. 1724.; and Muschenbroeck de tub. capill. cap. 4. exp. 12. cor. 2. and cap. 7.

§ Wepfer de apoplex. p. 277.; and Mead on poisons, edit. 3.

are thrown out by the exhaling arteries, along with the finer parts of the blood, into any cavity of the body) should again unite by their strong mutual attraction, so as to form globules of larger diameters than those of the absorbent veins, it is evident, that they could never be taken up by these vessels, but must remain for ever in such a cavity.

To conclude our observations on the absorbent vessels; it may not be amiss to take notice, that upon the internal surfaces of the follicles and secretory and excretory ducts of the glands, there are bibulous veins, whose office is to carry off those fluids which would be improper to enter into the several secretions. And, if we suppose these absorbent vessels, like other capillary tubes, to attract, according to their different natures, different fluids more or less strongly, we shall see one great cause of the various secretions performed in the bodies of animals.

II. O B-

II.

OBSERVATIONS

ON THE

SENSIBILITY and IRRITABILITY

OF THE

Parts of MEN and other ANIMALS.

Occasioned by

The Celebrated M. DE HALLER's late Treatise on those Subjects.

Spiritus intus alit; totamque infusa per artus
Mens agitat molem——

VIRG.

OBSERVATIONS

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The Celebrated M. DE HALLER'S late Treatise on these Subjects.

By
MRS. MARY HALLER.
LONDON:
Printed by J. B. ROBINSON, at the Theatre-French, in Pall-mall.

OBSERVATIONS

ON THE

SENSIBILITY AND IRRITABILITY.

OF THE

Parts of MEN and other ANIMALS.

PART I.

Of Sensibility.

THE truly learned and justly celebrated M. de Haller, in his late treatise *De partibus corporis humani sensibilibus et irritabilibus* *, has favoured the world with an account of many new and curious experiments; from which he has sometimes drawn such conclusions as, if just, must necessarily produce some considerable changes both in the theory and practice of medicine. Being sensible how contrary his doctrine is, in many things, to the received opinion of almost every physician, antient as well as modern, he has taken uncommon pains in making many and repeated experiments; as much to overpower the incredulous by their number, as to secure himself from any chance of being deceived †.

If the conclusions, I say, in that learned treatise shall be thought just, physicians and surgeons must treat some of their patients in a manner very different from what they have hitherto done; it seems therefore to be of some consequence to consider this matter with attention, and to examine particularly, How far M. de Haller's system of sensibility is, or is not, well founded.

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SECT.

* Acta Gottingenf. vol. 2. ad an 1752. page 114. &c.

† Acta Gottingenf. vol. 2. p. 115.

S E C T. I.

THE illustrious author, in treating of the sensibility of the several parts of the human body, reckons among the insensible parts, the tendons, *aponeuroses*, ligaments, *capsulae* of the articulations, *periosteum*, bones, marrow, *dura* and *pia mater*, *pleura*, *peritoneum*, *pericardium*, *mediastinum*, and *cornea*.

1. HE informs us, that living animals, whose tendons were cut, burnt, pricked, or torn, shewed no signs of uneasiness; and, when a little part of the *tendo Achilles* was left entire, they walked without any seeming pain *.

2. WHEN the ligaments and *capsulae* of the articulations were pricked with a needle, scraped with a knife, or had oil of vitriol or *butyrum antimonii* applied to them, the animal shewed no sense of pain †. The wounds of these parts and of the tendons were followed with no bad symptoms, and were cured without any other remedy than the *saliva* of the animal, and sometimes without it ‡.

3. THE *periosteum*, when wounded, torn, or burnt, caused no pain to the animals §.

4. HE allows feeling to the teeth, but not to the other bones, because they are not furnished with nerves, and because he has seen the skull trepanned, without giving pain, in persons who were possessed of all their senses §.

5. HE denies feeling to the marrow, not from any experiments of his own, but because it is a fatty substance without nerves **.

6. WHEN the *dura mater* was cut or lacerated, or burnt with oil of vitriol, spirit of nitre, and *butyrum antimonii*, the animal seemed to have no feeling of the injury ††.

7. WHEN the *pia mater* was burnt by touching it with *butyrum antimonii*, the animals neither cried, nor were convulsed; but, as soon

* Acta Gotting. vol. 2. p. 120.

† Ibid. p. 122. et 123.

‡ Ibid. vol. 2. p. 121. et 223.

§ Ibid. p. 123.

§ Ibid. p. 124.

** Ibid. p. 125.

†† Ibid. p. 126.

soon as the brain itself was wounded, the body of the animal was twisted and distorted with violent convulsions *.

8. THE *peritoneum*, *pleura*, and *pericardium*, when laid bare and cut, or otherwise irritated, produced no change in the animal †.

9. HE denies feeling to the *mediastinum*, not upon the authority of experiments, but because, like the *pleura*, it is a membrane and without nerves ‡.

10. HE reckons the *cornea* insensible, because its nerves cannot be demonstrated, and it is often pierced with a needle without pain ||.

BESIDES the insensible parts above mentioned, there are others which, according to M. de Haller, have either no sense of feeling, or a very obscure one; these are the arteries, veins, glands, and *viscera*, viz. the lungs, liver, spleen and kidneys, which, when pricked, cut, or otherwise irritated, shewed nothing like feeling §.

THE conclusions which the learned author draws from the above experiments, may be reduced to the three following.

1st, THAT the tendons, ligaments, *capsulae* of the joints, *dura mater*, *pleura*, and other membranes, are quite insensible.

2^{dly}, FROM the insensibility of these parts, and the difficulty of tracing, by dissection, any nerves to them, he concludes that they have none, and that this is the reason why they have no feeling.

3^{dly}, HE thinks it follows, That those parts which, from his experiments, he concludes to be insensible, have been unjustly accused by physicians as the seat of some painful diseases. Particularly, That the pain, swelling, and inflammation which have often followed bleeding in the flexure of the arm, have not been owing to the tendons or *aponeuroses*, in that part, being pricked by the lancet, but to the median nerve or some branch of the musculo-cutaneous nerves being wounded **: That we need not be afraid of wounds of the tendons, whether they be cut, pricked, burnt, or otherwise hurt: That the *cephalea* and *phrenitis* have not their seat in the *dura mater* ††: That the skin or subcutaneous nerves are the seat of

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* Act. Gotting. vol. 2. p. 130.

† Ibid. p. 130.

‡ Ibid. p. 131.

|| Ibid. p. 133.

§ Ibid. p. 131. and 132.

** Ibid. p. 121.

†† Ibid. p. 126.

the violent pain with which arthritic patients are affected, and not the ligaments or *capsule* of the joints *: And that the pain of the pleurisy has been without reason supposed to be owing to an inflammation of the *pleura*, which is void of feeling †.

IN the few observations which I propose to make on this doctrine, I shall, *first*, Consider the parts reckoned insensible by M. de Haller, in a sound natural state, such as they were in his experiments; and, *2dly*, When they are affected with diseases, whether in consequence of such experiments, or from other causes.

S E C T. II.

1. **I**N making or relating experiments, with a view to discover the sensibility or insensibility of the several parts of animals, particular regard should be had to an observation made by Hippocrates, *viz.* That a greater pain destroys, in a considerable degree, the feeling of a lesser one ‡; an observation which is confirmed by daily experience. Thus, pricking any part of the body so as to give considerable pain, will so obliterate the irritation in the left orifice of the stomach, which is the cause of the hiccup, as instantly to stop that convulsive motion. If a candle be brought near a person whose eyes are a little inflamed, it will give him uneasiness: but, if he be placed first in the sunshine, the candle will not add sensibly to his pain.

WHEN the hinder-feet of a frog are pricked, or otherwise wounded immediately after cutting off its head, it makes scarce any motions with its legs, and shews almost no signs of feeling; but, if the toes be pricked or cut ten or fifteen minutes after decollation, the legs and thighs are not only violently moved, but sometimes also the trunk of the body. Now, if in this case, as we see, the great pain occasioned by cutting off the head rendered the animal for some time insensible when its toes were wounded; is it to be wondered

* Aët. Gotting. vol. 2. p. 122. and 123.

† Ibid. p. 130.

‡ Aphor. lib. 2. No. 46.

wondered at, that, after the more sensible parts were cut, those animals which M. de Haller opened, shewed no signs of pain, when the less sensible parts were wounded?

WHEN the *thorax* of a living animal is laid open, it does not seem to receive any additional pain by pricking or cutting its heart; no new convulsions are produced, nor any change in the body, except perhaps a quicker repetition of the motion of the heart: does it follow from this, that the heart is without feeling? No, surely; but only that, after the torture suffered by laying open the *thorax*, the new pain produced by wounding the heart is too small to make any remarkable impression upon a dying and half insensible animal.

DOES it not appear, from what has been said, that a want of attention to the above mentioned maxim of Hippocrates may have given occasion to some mistakes with regard to the degree of sensibility in many of the parts of animals? Thus, it will not follow, that the tendons, ligaments, *capsule* of the joints, *periosteum*, and *dura mater*, are altogether without feeling, because no convulsive motions or other signs of acute pain appeared in the animals when they were cut, pricked, or torn; for this might be owing either to their not being endowed with any painful feeling, or to the greater pain occasioned by cutting the skin, subcutaneous nerves, &c. in order to get at those parts the sensibility of which our learned author proposed to try. I presume therefore to think, that the conclusion which should be made from his experiments, is, not that the parts above mentioned are wholly without feeling, but that they are much less sensible than many others, or than has been commonly believed by physicians.

2. WITH regard to the marrow which M. de Haller reckons insensible; Duverney's experiments made on men *, (which have also succeeded with my learned friend and colleague Dr Monro senior), and particularly his experiment made on a living animal before

* Dans les hôpitaux, où voyant panser ceux qui avoient eu un bras ou une jambe coupée, je pouvois voir la moëlle à decouvert,—toutes les fois que je la faisois toucher un peu rudement, le malade donnoit aussitôt des marques d'une nouvelle douleur. *Memoires de l'Acad. des Sciences* 1700. edit. 8vo, p. 255.

fore the Royal Academy of Sciences at Paris *, are a proof that this part is far from wanting feeling: and the reasons given by M. de Haller for his placing it among the insensible parts, seem not to be sufficient to balance those experiments. For the feeling of the marrow is not owing to its oil, but to the membranes containing this oil; and the experiments which demonstrate its sensibility, prove that those membranes are furnished with nervous filaments, although they may be too subtle for the knife of the most accurate anatomist.

3. THE *tunica cornea* is so far from being insensible, as M. de Haller believes, that any one may be soon convinced of the contrary by an experiment upon his own eye; for, when the *cornea* is touched with the finger, a sensible pain is felt. and it is well known, that powder of tobacco, or any acrid liquor applied to the *cornea*, excites a very acute sensation. Tho' the sclerotic coat of the eye is far from being void of feeling, yet I have found it to be less

* “ Vous vous souviendrez, Messieurs, que je fis scier devant vous, par le milieu, l'os de la cuisse d'un animal vivant; et, ayant fait ôter les chairs et les membranes, pour laisser le bout de l'os entierement à nud, comme tous ces ebranlemens et ces divisions caufoient de douleur tres cruelle a l'animal, j'eus la precaution d'attendre que cette douleur fût passée, et, quelque tems après, plongeant un filet dans la moëlle, vous vites que l'animal donna aussi-tôt des marques d'une tres vive douleur, ce que fut reiteré plusieurs fois avec la meme precaution, et avec la meme succès.”

Memoires de l'Academie Royale des Sciences 1700, edit. 8vo, p. 256.

M. de Haller, in answer to the authority of Duverney which I had cited, says, that “ a single experiment is not sufficient to prove the sensibility of the marrow, which is evidently cellular, and whose nerves have not yet been discovered (a).” But in confirmation of the truth of Duverney's experiments, I mentioned that they had also succeeded with Dr Monro senior; and have now to add, that Dr Al. Monro junior, having been present last summer (1760) at the amputation of the arm above the elbow, in a man of about forty, on account of a gun-shot wound in the hand, and a supervening mortification, after the arm was taken off, he pressed upon the bone, the marrow, and the muscles repeatedly with the point of his finger; when the bone was touched the patient felt nothing; touching the muscles occasioned little pain; but he complained considerably as often as the Doctor pressed the marrow with his finger.

(a) Memoires sur les parties sensibles, vol. 4. p. 109.

less sensible than the *cornea*, by touching both, not only with my finger, but with a bit of soft silk or linen *.

HAVING been lately present at the extraction of the crystalline lens in Mr Sharp's method †, I inquired particularly of the patient, whether he felt any pain when the *cornea* was first pierced with the knife? he told me he thought the pain was much the same with what he used to feel when the skin of his arm was cut in bleeding. It ought however to be remarked, that though the skin and *cornea* have both a considerable degree of sensibility; yet, when they are cut quickly with a very sharp instrument, there is less pain felt than one would imagine. Thus, when the skin is slightly wounded in shaving, the blood that follows is often the first thing that lets one know of the accident: and this, together with the pain occasioned by holding the eye firm in its orbit, and the concern the patients are generally under, may well account for their being sometimes scarce sensible of any pain when the *cornea* is pierced with a sharp needle. But upon the whole, it appears, that the *cornea* is possessed of a remarkable degree of sensibility; and consequently, that M. de Haller's

* M. de Haller remarks, that in these experiments, I only touched the *conjunctiva* (a), which is certainly true; nor was I ignorant of this. But as M. de Haller had pronounced the *cornea* in general to be insensible, and had made no exception in favour of the *conjunctiva* which covers it, my experiments were certainly in point, and the conclusion from them just: and it will be found very difficult to prove by any experiment, that the pain occasioned by cutting the *cornea* is not partly owing to this membrane, as well as to the *conjunctiva*.

M. de Haller, unwilling to allow sensibility even to the *conjunctiva*, ascribes the pain occasioned by touching the *cornea* to small branches of the fifth pair of nerves which run between these membranes. But supposing the existence of nervous branches between the *cornea* and *conjunctiva*, as well as between this last and the sclerotic, altho' no anatomist has yet demonstrated the former; yet the pain occasioned by touching the *cornea* very gently, or the sensation produced by the cool air blowing on it, cannot well be conceived to be owing to any thing else than the sensibility of its exterior covering. Nay, if the *cornea* itself were not more sensible than the sclerotic, why should the *conjunctiva* feel more acutely where it covers the former, than where it is contiguous to the latter? The *conjunctiva*, where it covers the *cornea*, is certainly one of the most sensible parts of the whole body, and least able to bear any hurt, or the application of any acrid substance. Nor could its sensibility be so great, if it were owing solely to some branches of nerves running between it and the *cornea*.

† Philosoph. transact. vol. 48. p. 1. p. 322.

(a) Memoires sur les parties sensibles, vol. 4. p. 59. and 108.

Haller's position, That all membranes are void of feeling *, must admit, at *least*, of one exception †.

4. THE learned author allows to the kidneys either no feeling at all, or an obscure degree of it; because he could observe no signs of pain in the animals whose kidneys he cut or pricked; but, after cutting the skin, the abdominal muscles, &c. and displacing the intestines in order to get at the kidneys, it was scarcely to be expected, that the animals would then shew any signs of additional pain when those last parts were wounded, unless they had been equally, or more sensible than the parts first dissected.

A physician of my acquaintance, who had an opportunity of seeing the operation of nephrotomy, was told by the patient that, when the kidney was opened, he felt pain, tho' duller and less acute than when the skin was cut.

ONE instance of this kind is more decisive in favour of the sensibility of the kidneys, than twenty experiments on brutes, who cannot inform us whether they feel a slight pain or none at all; and, if the kidneys be less sensible than the skin, we cannot expect that wounding them will add considerably to the pain which the animals suffered before from cutting the skin and muscles.

IT is observable, that, while M. de Haller denies feeling to the kidneys, he allows it to the ureters: not because animals, when these are cut or wounded, shew signs of greater pain than when the kidneys are treated in the same manner; but because he supposes the ureters to be of the nature of the skin, and propagated from it ‡. And indeed, no experiments upon brute animals would have been sufficient to have proved the ureters insensible in men, when stones passing from the kidneys to the bladder generally occasion such exquisite pain. But, does not the acute pain attending a *nephritis*,
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* Aët. Gotting. vol. 2. p. 130.

† M. de Haller represents me as inconsistent with myself in giving the *cornea* as an instance of a membrane that is sensible, after having owned that, when pierced with a sharp needle, it often occasions little pain (a); but there is really no inconsistency here, since I have observed, at the same time, that the skin, which is among the most sensible parts, feels generally less from being slightly cut with a sharp razor, than the *cornea* does when pierced with a needle.

‡ Aët. Gotting. vol. 2. p. 131

(a) Vid. Memoires sur les parties sensibles, &c. vol. 4. p. 108.

and which is sometimes occasioned by a stone lodged in the kidneys, shew, that they are endowed with feeling as well as the ureters? while nothing can be concluded from *calculi* lying long in the kidneys without giving pain *, except that they were so placed as not to hurt them.

5. ALTHO' brute animals shew little sensibility, when their glands are pricked, or have acrid things applied to them, immediately after the cutting of their skin; yet, we know that a bruise of the testicles often causes, instantly, such exquisite pain as to make men faint; and that a blow on the breast of a woman often excites shooting pains in the gland there, though no mark of the bruise appears in the skin. These seem to be such proofs of the sensibility of the glands, as no experiments made on brute animals will easily overthrow.

6. M. DE HALLER allows the membranes of the *aorta* near the heart, and of the temporal, lingual, labial, thyroid, and pharyngean arteries, to be sensible; but believes that the coats of the arteries in other parts of the body have either no feeling at all, or a very small degree of it; though it does not appear from his experiments, that animals shewed more sense of pain when the former than when the latter were irritated. In this case, he seems to have made no experiment, but rests his opinion on his tracing nerves to the former, which he could not do to the latter: an argument he makes use of upon other occasions, and which is next to be examined.

7. As the learned author not only forms his opinion of the insensibility of many parts of the body upon experiments made on living animals, but also on their want of nerves; we shall briefly consider, whether, from the real or seeming insensibility of any part, or from anatomists being unable to demonstrate its nerves, we are allowed to conclude that it has none.

ALTHO' the tendons are quite insensible, according to M. de Haller, and their nerves can scarcely be shewn by anatomists; yet we are convinced that the tendons have nerves, from the following observation. In foetuses and new-born children, many parts which

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* Act. Gotting. vol. 2. p. 132.

afterwards, in an adult state, become tendinous, are then muscular, or partly so; and as animals advance in age, the proportion of the tendinous to the muscular part gradually increases: we must either, therefore, deny nerves to the muscles, or allow them to the tendons also *.

ALTHO' we cannot trace nervous filaments to the small arteries, we have reason to believe that they are furnished with them; else, how could the distension of their coats in inflammations occasion such acute pain †? I think we may conclude every part that is liable to be inflamed by irritation to be, in some degree, sensible and possessed of nerves; for, since the inflammation cannot in this case be owing to any increased force of the heart, the distension of the small arteries, and the greater *impetus* of the blood in them, must be owing to an increased oscillatory motion in the vessels themselves, excited by the unusual irritation: but these motions of the small vessels being of a like kind with those alternate contractions which are observed in muscles whose fibres have been irritated, it will follow that those vessels partake of a muscular nature, and consequently have nerves like the other muscles.

WITH regard to the membranes; since the *dura mater* and *pleura* are furnished with nervous filaments, which anatomists have been able to demonstrate ‡, we may reasonably conclude that the other membranes are not destitute of them; although they may be too small to come under the eye of the best dissector: this is certainly true

* The reader will observe, that from those parts which were once muscular becoming afterwards tendinous by age, I only conclude that they are not destitute of nerves, but not that they are sensible; which, however, M. de Haller has inadvertently represented me as saying (a). This would have been contrary to my acknowledgement of the very obtuse feeling of the tendons, and to what I have said below, of the sensibility of the parts depending not solely on their having nerves, but on the disposition and state of those nerves.

† M. de Haller seems sometimes to allow sensibility to the arteries, and at other times to deny that they have any; because, when they are tied, the animals do not seem to feel any pain. But it is to be observed, that the ureters, whose sensibility he acknowledges, may be tied or wounded with as little signs of pain as the arteries. Vid. *Memoires sur les parties sensibles*, vol. 4. p. 87. et 110.; and *Act. Gotting.* vol. 2. p. 142.

‡ Winslow Exposition. anatom. sect. 9. No. 35. and sex. 10. No. 47.

(a) *Memoires sur les parties sensibles*, vol. 4. p. 103.

true of the *cornea* and the membranes containing the marrow, which we have shewn to be sensible, and consequently not without nerves. It appears, therefore, that we cannot conclude any part to be insensible, merely because its nerves cannot be discovered.

ON the other hand, it is allowed, that we cannot certainly conclude, from a part's being furnished with nerves, that it is therefore sensible, and to what degree: for the nerves must be in a certain state to perform their functions rightly; and, in proportion as they recede from this, their sensibility will be more or less blunted.

THE bones, for instance, which in a natural sound state are insensible, are nevertheless furnished with nerves; as appears from the great sensibility of the granulated substance which rises from them after fractures, or after being chizelled, or when they exfoliate: this soft flesh, however, gradually loses its feeling as it grows harder, till being at last turned into a callous or osseous substance, it becomes wholly insensible.

THE membranes of the *tela cellularis* are, in a natural state, soft, flexible, and ductile, and have little feeling; but, in every wound or ulcer, when they acquire some more firmness, they are sensible of every touch and every acrid application. After a cicatrice has for some time covered the parts where the sore was, and they have returned to their natural softness, these cellular membranes lose again their sensibility, as appears on making a new wound through the cicatrice; and recover it again, whenever they become firm and tense, by the new inflammation and suppuration.

THE *dura mater*, which, in a sound state, has but little feeling, granulates after the trepan, and feels every irritating substance applied to it; and the same thing happens to cartilages, ligaments, tendons, membranes, &c.

WITHOUT attention to this change in the firmness of parts, and its effect upon their nerves, we could never account for what has been observed above, viz. that the parts of muscles, which in foetuses and children are lax contracting fibres and very sensible, become in a great measure insensible, in a sound state, when, by the

age of the animal, they are compacted into tendons, as happens to many of them.

IF sensibility, then, be a sure mark of the existence of nerves in any part of the body, there is none without them, altho' anatomists will never be able to demonstrate them in every part.

FROM what has been said, it will appear, that M. de Haller's curious experiments on living animals do not sufficiently prove the whole doctrine which he would deduce from them; and that his argument for the insensibility of parts, drawn from their nerves not being demonstrable, is not altogether conclusive. Let us next see what further light diseases will throw upon this subject.

S E C T. III.

IF the parts reckoned insensible by the learned M. de Haller were really without nerves, it would follow, that they could in no case become the seat of painful sensation; and even supposing them furnished with nerves, but possessed only of a small degree of feeling, it may be thought, at least, improbable that they can be the seat of those painful diseases commonly ascribed to them. In order to set this matter in a just light, it will be proper to distinguish between parts in a sound and in a diseased state. In a sound state, the feeling of many parts of the body is but dull, and this is necessary to prevent the uneasiness we should otherwise suffer, when our organs are stretched, pressed upon, &c. in the common offices of life: such parts, therefore, when cut or wounded, in a sound state, give little uneasiness; but, if afterwards an inflammation comes on, they become extremely sensible, and their over-stretched vessels and nervous filaments occasion acute pain, by which we are excited to endeavour to remove the complaint.

IT is certain, that those parts which are most sensible in a sound state, acquire a more acute feeling when inflamed. Thus the stomach, which, in health, can bear wine, brandy, and other pungent liquors, without being hurt, is, when inflamed, often brought
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into convulsions by the mildest. And light, which gives so much pleasure to the eye in a sound state, becomes intolerable when that organ is inflamed. Nor can we doubt that the more insensible parts may acquire, when inflamed or otherwise diseased, a great degree of sensibility. The instances above recited have shewn this to be true of the bones, *tela cellularis*, and *dura mater*; and the following facts will, I believe, evince the same thing, in other parts, which have been accounted either wholly, or almost wholly, insensible by the learned author.

As often as there is inflammation, especially when tending to suppuration, in any of the glands, as the parotids, tonsils, maxillaries, *mammæ*, *testes*, kidneys, &c. the patient often complains of great pain, before the teguments are inflamed, or even considerably stretched. And is not this a better proof of the sensibility of those parts, than *schirri* and other indolent swellings are proofs of the contrary?

THE fore-part of the eye, when inflamed, can bear the touch of no hard or acrid substance; and *fungi* rising from it occasion very sharp pain, when fretted.

IN the rheumatism, the joints, where the skin is unstretched and of the natural colour, and where there are no muscular fibres, are severely pained on the least motion. This I should refer to the sensibility of the ligaments and tendons; since large branches of nerves, thus affected, would occasion convulsions of the muscles they go to, which does not happen: Besides, in such cases, the pain is not felt where the large nerves are.

A contusion, by a fall on the great *trochanter* of the thigh, without causing an *ecchymosis*, or a swelling of the teguments, often brings on, in a little time, racking pain on all the outside of the thigh, leg, and foot, continuing for months or years through the whole extent of the *fascia lata*.

AN inflammation of the *periosteum*, as in the *panaris*, where the suppuration happens between this membrane and the bone, nay even the fulness of the vessels of an overstretched *periosteum*, (as by heat or food in venereal nodes), gives very sharp pain. And, in
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the *spina ventosa* and other suppurations of the marrow, pain is felt before any signs of the disease appear externally.

THESE observations seem to demonstrate, that many of those parts which M. de Haller judges to be insensible, are often the seat of considerable pain in the human body; and, I suspect, that, in other examples, where that learned and ingenious author endeavours to assign a different seat of the painful sensation, he is mistaken, and, perhaps, unawares, is laying the foundation of a hazardous practice.

1. THUS he imagines that the pain, swelling, and inflammation of the arm, which have sometimes followed the opening of the median vein, must have proceeded, not from a wound of the tendon of the *biceps* muscle, but from one in the median or some other nerve. But, if this were true, why should not the like symptoms sometimes follow bleeding in the cephalic or jugular veins? In opening the jugular vein, some nervous filaments are frequently wounded and occasion a sharp pain, as if the point of the lancet had been left in the wound; this, however, goes off in a day or two, without any bad consequence. But the mischiefs which have followed bleeding in the median vein are different. Though little or no pain is felt at first, yet afterwards, not only the whole arm is violently pained and swelled, but a particular hard tumour is often formed in the place where the wound was made, from which oozes a thin lymph; the patient does not recover the full use of his arm for several months; nay, sometimes he loses the motion of the elbow-joint. Now that a wound in the tendon is, sometimes at least, the cause of those symptoms that follow bleeding in the arm was evident, in a patient who died here some years ago, of a fever occasioned by the pain, tumour, and inflammation consequent upon opening the median vein of the right arm; the *tendo bicipitis* of which was swelled to almost ten times its natural bulk.

How very sensible tendons may become, when inflamed, appears from various observations; particularly from one mentioned by the learned Baron Van Swieten, of a Nobleman who was seized with violent convulsions over his whole body the moment his surgeon

geon took hold of one of the tendons near his ankle, mistaking it for a part of the fatty membrane *.

2. THE learned author refers the pain of the gout to the skin or subcutaneous nerves, and not to the *capsule* or ligaments of the joints affected. But does not the rigidity of the joints, which the gout at last produces, shew, that its seat is deeper than the skin or subcutaneous nerves; and that the seat of the disease is in the ligaments of the articulations, and tendons of those muscles which are inserted near them?

IN spraining the wrist or ankle, we do not always feel great pain immediately; but soon after, when the parts begin to swell and inflame, a considerable pain ensues, which is increased if the joint be moved. Does not this pain proceed chiefly from the over-stretched ligaments or tendons? surely it cannot be owing to any hurt received by the skin or subcutaneous nerves. And, if the ligaments or tendons may be thus affected by being too much stretched, why may not they be the principal seat of the gouty pain?

CHALK-STONES in a joint are frequently the cause of sharp pain before they pierce the capsular ligament, and before the skin becomes much stretched or red. Further, without allowing sensibility to the ligaments, how shall we explain what my learned colleague Dr Monro senior, and, I dare say, many others have often-er than once seen? An issue, for a dropsy of the knee, made with a caustic or a knife, and dressed with the pea for a considerable time, created little uneasiness; but, after a puncture by a lancet made, near to where the issue was, through the *capsula* of the joint, to let out the water, such racking pain and inflammation ensued as brought the patient to the brink of the grave †.

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* Comment in Aphor. Boerhaave, vol. 1. p. 241.

† In answer to this, M. de Haller has thought proper to remark, that Mr Warner, who is much better acquainted with surgery than Dr Whytt, recommends the opening the capsular ligament as the only effectual cure in a dropsy of the articulations (a). The impropriety of this remark must appear in a strong light to the reader, when he observes that what I assert here is solely upon the authority of Dr Monro senior, whose skill in surgery, as well as his accuracy in making observations, is too well known to be called in question.

(a) Memoires sur les parties sensibles, vol. 4. P. 59.

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3. M. DE HALLER is of opinion, that the insensible *dura mater* cannot be the seat of a headach or of a *phrenitis*. But how little sensible soever this membrane may be in a natural state, yet, if it may be affected with pain as often as it is inflamed or obstructed, it may still be the seat of those disorders. In those who have died of a *phrenitis*, the *dura* and *pia mater*, as well as the cortical substance of the brain have been found inflamed, suppurated, and mortified: and in those who, after recovering once and again of a *phrenitis*, have died of other diseases, the *dura* and *pia mater* have been found thicker and harder than natural *.

As the headach in fevers often begins several days before a *delirium*, we cannot ascribe it to an obstruction in the cortical part of the brain, but in the *dura* or *pia mater*. Nor can this headach have its seat in the teguments of the skull; otherwise the pain would be increased by pressing the part affected, as often happens in those periodical headachs which seem to have their seat in the subcutaneous nerves, or *pericranium*.

Lastly, M. de Haller thinks, that the intercostal muscles, or large nerves running between the ribs, are the seat of the pain in the pleurisy, and not the *pleura* itself, which, according to him, is insensible. But, if this membrane, notwithstanding its small degree of sensibility in a sound state, may be affected with great pain when inflamed, it will hardly be doubted that it is sometimes the seat of the pleurisy; since, in those who have died of this distemper, the *pleura* has been found inflamed and suppurated †.

BUT, besides the insensibility of the *pleura*, he advances another plausible argument to prove, that the pleurisy can never have its seat in this membrane, viz. the patient's feeling the greatest pain in inspiration when the ribs are brought nearer each other, and conse-

But further, the pains which followed the opening of the capsular ligament, in the only case which is mentioned by Mr Warner, though they were less violent than is usual in such cases (a), shew that this ligament, however little feeling it has in a sound state, may, when diseased, become the seat of painful sensation.

* Van Swieten Comment, in Boer. Aphor. vol. 2. p. 604.

† Van Swieten Comment. vol. 3. p. 8.

(a) Vid. Philos. transact. vol. 49. p. 457.

consequently when the *pleura* is less upon the stretch than it was in the time of expiration. But the learned author has long ago justly observed, that ordinary and gentle inspiration in men, is chiefly performed by the diaphragm, while the intercostal muscles are scarce employed at all *; and therefore in inspiration, which action pleuritic patients perform with difficulty, the ribs may be supposed to alter their situation very little; but, as the inferior part of the *pleura* must be somewhat stretched by the descent of the diaphragm in inspiration, it is no wonder that the pain should be then most acute.

IN women, especially such as are pregnant, who use the intercostal muscles more in ordinary inspiration than men, the *pleura* will be more stretched at that time than during expiration; because the cavity of the *thorax* is increased in wideness and depth, as well as length.

WITH regard to what M. de Haller says of the ribs approaching each other in inspiration; though this is certainly true of the superior ribs, yet I have some doubt, whether it be so in the inferior ones: for, in a full inspiration, I can with my fingers plainly feel the six or seven inferior ribs recede from each other, and approach again in the succeeding expiration †. Therefore it must appear, that the increase of the pleuritic pain in time of inspiration can be no proof, that the disease has not its seat sometimes in the *pleura*.

UPON the whole, although these curious experiments shew, that several parts of animals are possessed of a much more obscure degree of feeling than has been commonly imagined; yet I should think, that the reader, after weighing what has been said, will not pronounce them altogether insensible.

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PART

* Praelect. in Institut. med. Boerhaav. vol. 4. No. 615. not. (a).

† The reason why not only the false ribs, but also some of the true ones, rather recede from than approach each other in inspiration, may be understood from what is briefly said concerning the motions of the *thorax*; by Dr Monro, in his Anatomy of the bones, edit. 5. P. 242.

PART II.

Of Irritability.

SECT. I.

ALTHOUGH many of the parts composing the human body are endowed with a considerable degree of elasticity, whereby they restore themselves when over-stretched; yet muscular fibres alone are possessed of a peculiar contractile power, which they exert in consequence either of an effort of the will, or of some *stimulus* applied to them or their nerves: by the former, voluntary motion is produced; by the latter involuntary *. The illustrious M. de Haller, who calls the contractile power of irritated muscles by the name of *Irritability*, has, by a variety of curious experiments upon living animals, shewn, that it is a property of all muscular fibres; and that no part, which is not muscular, is irritable, although, of the muscular parts, some are more and others less sensible of irritation. But when, in his enumeration of the parts of the body that are or are not irritable, he allows irritability to the lacteal veins, mucous glands, and sinuses, and yet denies it wholly to the kidneys and ureters, and almost wholly to the arteries, veins, and excretory ducts of the glands, we cannot help differing from him: since these last parts are, at least, as much muscular as the former; and since his own experiments on living and dying animals shew neither the one nor the other to be irritable †.

THAT the small arteries are not without irritability, may be demonstrated by experiments. Thus, when an acrid cataplasm is applied to the skin, or spirit of wine to the eye, whence proceeds the inflammation which is soon produced in the skin, and almost instantly

* Vid. an Essay on the vital motions, &c. sect. 1. and 10.

† Act. Gottingenf. vol. 2. p. 139.—143.

stantly in the eye? Not, surely, from any increased force of the heart or larger arteries, but from the irritated vessels themselves, which are agitated with alternate vibratory contractions; by means of which the *momentum* of the blood in them is much increased, and red globules are pushed into those vessels which, in a sound state, only receive the *serum* or the lymph *.

NOR can we conclude that the arteries are destitute of irritability, because the *aorta* was not observed to contract itself when pricked with a sharp instrument, or touched with acrid liquors †; since the same is true of the mucous glands and sinuses, which yet the learned author allows to be irritable ‡. And it is not improbable, that the small capillary arteries may be more irritable than the *aorta* or larger ones; because their muscular coat, as it is called, is much less firm and tendinous.

FARTHER, M. de Haller judges the lacteal veins to be irritable, because, after death, they contract so as to expel the chyle and become invisible §: But do not all the arteries of the body, small as well as great, also contract themselves after death, and push most

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of

* M. de Haller says, that the irritation of the small arteries, if they are hollow muscles, ought rather to empty them than increase their diameter (a). Now, if an irritated artery could empty itself as easily as the bladder of urine, and were as slowly supplied with new fluids, an irritation of it would have the effect mentioned by my learned opponent. But since the arteries are furnished with a continued supply of blood from the heart, it is easy to see, that as often as by any considerable *stimulus* the alternate contractions of the small arteries of any part are greatly increased, the force of the blood must not only be augmented, but these arteries, as well as the smaller lateral branches which they send off, must be enlarged in their diameters, and contain a greater quantity of fluids than usual, *i. e.* the part will be inflamed.

The increased heat, redness, and pulsation in an inflamed part can only be conceived to happen, either from an increased force of the heart and larger arteries, or of the small vessels themselves. But we know that in many local inflammations from external causes, the force of the heart and large arteries is not altered; therefore the inflammation must in such cases be ascribed to the increased alternate motion of the small vessels themselves; for their continued spasmodic contraction would occasion a sense of cold and a paleness, not a heat and redness.

Lastly, Since an irritation of the salivary and lachrymal vessels, and of the mucous ducts and sinuses, increases the motion of the fluids in them, it will be difficult to give a reason why it should not have a similar effect in the small arteries every where through the body.

† Aët Gottingenf. vol. 2. p. 141.

‡ Id. p. 143.

§ Ibid. p. 142.

(a) Memoires sur les parties sensibles, &c. vol. 4. p. 113. et 114.

of their blood forwards into the veins? And is not this contraction of the lacteals owing more to the elasticity of their coats now increased by cold, than to a peculiar muscular contraction? However, if the lacteals be irritable, as is probable, (though for other reasons than the one now mentioned), it will follow, that the lymphatic and other vessels of the body are so likewise: for the lacteals are only a kind of lymphatic veins arising from the villous coat of the intestines, which, on account of the whiteness of their fluid, are so called. Nor have we any reason, from their muscular structure, to ascribe irritability to the lacteals and thoracic duct, more than to the other vessels of the body.

WITH regard to the veins, I shall only observe, that, since the alternate contractions of the trunks of the *venæ cavæ* near the heart, shew them to be possessed of a great degree of irritability; it is not probable that the other veins are wholly without it. M. de Haller indeed denies any peculiar motion to the *cava*, and ascribes its seeming alternate dilatation to the blood pushed back into it by the contracting auricle *. But, if this were true, how could the *cava* contract five or six times before the right auricle performed one pulsation, as Steno has observed in rabbits †? or how could it continue its alternate motions, not only for a considerable time after the right auricle had ceased to move ‡, but even after the heart, with that auricle, was separated from it §? These facts seem so clearly to shew that the motions of the *venæ cavæ* do not proceed from the alternate contractions of the right auricle, as to make any further observations on this matter unnecessary §.

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* *Primæ lineæ physiolog.* edit. 2. No. 113.

† Bartholin. *Epist. med.* cent. 4. p. 3.

‡ Bartholin. *Epist.* cent. 4. p. 110.; and *Essay on vital and involuntary motions*, p. 53. 54.

§ Wallæus de motu sang. ad fin.; *Anatom. Bartholin.* p. 783.

§ M. de Haller, in his later writings, acknowledges the motions of the *vena cava* to be owing to its own fibres; and has further shewn that the pulmonary veins near the left ventricle of the heart are endowed with the same power: But from his remarks on this passage, the reader would imagine that M. de Haller had never ascribed the motion of the *vena cava* to its dilatation by the blood pushed back into it by the right auricle, and that I had charged him unjustly with this opinion (a); which nevertheless is to be found in No. 113. of the second edition of his *Primæ lineæ physiolog.*

(a) *Memoires sur les parties sensibles*, vol. 4. p. 116. 117.

Does not the sudden flow of pale urine in hyfteric cafes, and of the *saliva* in a hungry person upon the taste or even upon the sight of grateful food, shew that the secretory vessels of the kidneys and excretory ducts of the salivary glands are, in such cases, agitated with an unusual oscillatory motion, and consequently that they have irritability? Nor ought M. de Haller to have denied this power to the vessels of the kidneys and excretory ducts of the glands: since he allows it to the lachrymal glands and mucous sinuses, because they pour forth their fluids more copiously when stimulated; although his experiments discovered no signs of irritability in them *.

When a stone passes from the kidneys to the bladder, does not the irritation of the sharp stone occasion some spasmodic contraction in the ureter? and does not a large dose of *opium* facilitate its passage, by abating or destroying the painful feeling, and consequently lessening the constriction of the ureter? This canal, therefore, seems to be possessed of some kind of irritability, altho' M. de Haller informs us that in the animals which he opened, it was insensible of the *stimulus* of oil of vitriol † ‡.

If the learned author's experiments discovered no kind of irritability in the blood-vessels, lacteals, glands, and mucous sinuses, it will not follow that the *iris* has not that power, although it did not seem to contract when irritated with the point of a knife ||.

He adds, that the dilatation of the pupil cannot be owing to any muscular power, because it becomes widest at death or immediately after §. I have elsewhere observed that the dilatation of the pupil is owing to the longitudinal fibres of the *uvea*, which, by their natural contractility, draw back its edges, when the orbicular muscle is not excited into contraction by the action of light on the *retina* **: at death, therefore, when the eye is insensible, the pupil becomes wide;

* Act. Gotting. vol. 2. p. 143.

† Ibid. p. 142.

‡ As a further proof of this, my ingenious Colleague Dr Alexander Monro junior informs me, that in a pig which was strangulated and half dead, he observed the ureter to contract very remarkably, when he touched it with the point of his finger; nay, when he moved the point of his finger along the surface of the ureter, a successive contraction of that canal was produced from above downwards.

|| Act. Gotting. vol. 2 p. 143.

§ Ibid.

** Essay on vital motions, sect. 7.

wide; but, some time after death, as Winslow has always observed *, (and I have also seen), the pupil grows narrower; because the longitudinal fibres of the *uvea* losing their tone, become lax and extended †. Nor does M. de Haller seem to have attended to what is said in page 70. and 71. of my Essay on the vital motions, &c. when he mentions the dilatation of the pupil at death, as a proof that it is not owing to the contractile power of the fibres of the *uvea*; since this very dilatation of the pupil, compared with its contraction some time after death, demonstrates the truth of what I have advanced. But, after all, if the dilatation of the pupil be not owing to the elasticity or natural contractility of the radiated fibres of the *uvea*, to what cause can it be ascribed? For it is presumed, that the learned author does not insist on the aqueous humour pressing the edges of the pupil outwards, as being contrary to the laws of hydrostatics. It may not, however, be improper to observe, that, although we should suppose the *uvea* to be, strictly speaking, not muscular, but only a cellular membrane; yet, like the *dartos* of the *scrotum*, it would, by its elasticity, draw back the edges of the pupil as soon as the cause contracting it ceased to act. And although, at the time of death, the pupil would on that account be much enlarged, yet some time after, when this cellular substance begins to lose its elastic power, the pupil would become narrower.

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* Memoires de l'Acad. des sciences 1721. edit. 8vo, p. 416.

† M. de Haller mentions in several of his experiments, that he observed the pupil very wide in animals not only at the time of death, but for some hours after it: Nor is this inconsistent with Winslow's having almost constantly found the pupil of a moderate size in the human body a day or two or perhaps longer after death; for the pupil, which continues wide for several hours after death in men, as well as in other animals, becomes narrower as soon as the fibres of the *uvea* have lost their tension and elasticity: Nay, I have observed the pupil, after having been remarkably wide and without motion for some time, become narrow even a day or two before death in two boys who died of a dropsy in the ventricles of the brain. In these patients, sp. of sal. ammon. held to the nose, or a spoonful of a cordial julep, made the pupil instantly as wide as it is observed to be in a confirmed *gutta serena*; but soon after, it became narrower again; which seems to be a proof that the dilatation of the pupil, in this case, was owing to a contractile power communicated to the fibres of the *uvea*, by the stimulus of the volatile spirits or cordial; while its contraction afterwards was only the consequence of the *uvea* being more relaxed.

M. DE HALLER, because he cannot discover any orbicular muscle surrounding the edge of the pupil, concludes that there is none; and ascribes the contraction of that part to a stronger influx of fluids into the small vessels of the *uvea*, occasioned by the *stimulus* of light acting upon it. My reasons for not admitting this hypothesis I have shewn elsewhere *; and therefore shall only add, that as we conclude from the various motions of many of the smaller insects, that they are, as well as larger animals, furnished with muscles, though we can neither demonstrate those instruments of motion by the knife, nor by the microscope, so we may infer the existence of the orbicular muscle of the *uvea* from the regular motions of the pupil, although its texture may be so delicate as scarcely to be distinguished by the anatomist from a denser kind of cellular membrane.

I shall now proceed to consider what the learned author has offered concerning the nature of irritability †.

S E C T. II.

IN my Essay on the vital and involuntary motions of animals, I had endeavoured to shew, that *stimuli* applied to the muscles of animals

* Essay on vital motions, p. 69. &c.

† M. de Haller has by mistake represented me as saying, That the contraction of every muscle of the body is interrupted with alternate relaxations (*a*): whereas, in p. 12. 13. 126. &c. of my Essay on the vital motions, I have expressly excepted the *sphincter pupillæ*, muscles of the internal ear, and some others, whose contraction is owing to a *stimulus* acting on some neighbouring or distant part. I have indeed affirmed, That those muscles to whose fibres a *stimulus* is immediately applied, are always agitated with alternate contractions and relaxations; and M. de Haller himself agrees with me in this, when he says, that all muscles, not so much as one excepted, that he knows of, tremble and palpitate after death, and are alternately contracted and relaxed (Aët. Gotting. vol. 2. p. 139. and 144). The bladder of urine, however, seems to differ from the other muscles or muscular organs in contracting uniformly and without any alternate relaxations when it is pricked, or exposed to the cold air, in animals that are dying or newly dead (*b*). In this respect, the bladder resembles the *dartos scroti*, which is excited into a continued contraction, and furls up the *scrotum*, when cold water and astringent or acrid liquors are applied to it, or when the skin of the *scrotum* is gently irritated by titillation.

(*a*) Aët. Gotting. vol. 2. p. 145.

(*b*) Aët. Gotting vol. 2. p. 142. and 145.

animals excite them into contraction, by producing an uneasy feeling in them or their nerves ; but M. de Haller, who judges irritability to be an innate property of muscular fibres, is of opinion that it depends not upon the nerves, and has no connection with sensibility :

1. BECAUSE the most sensible parts, such as the nerves and skin, are not irritable *.

2. BECAUSE the irritability of our organs is observed not to be in proportion to their sensibility †.

3. BECAUSE some parts which have no feeling are irritable ‡.

WITH regard to the first of these, since muscles are the only organs of the body which, by their peculiar structure, are fitted for motion, it is no wonder that the nerves should have no irritability, since the want of it is only a necessary consequence of their make ; for a power of contraction does not depend on sensibility alone, but upon that in conjunction with a particular form.

IN answer therefore to the first argument, I should say, that, although irritability always infers some degree of sensibility, yet sensibility does not infer irritability, unless the part be, by its structure, fitted for motion, *i. e.* in other words, unless it be what we call muscular ||.

ALTHOUGH the skin is not irritable in the same sense that the muscles are, yet the inflammation and pain raised in it by blisters and other acrid applications shew, that it is readily fretted or irritated by *stimuli*. The skin, when stimulated, is not brought into alternate contractions, because it is not by its structure made capable of this kind of motion ; but it becomes red, is inflamed, and pours forth

* Act. Gotting. vol. 2. p. 134.

† Ibid. p. 136.

‡ Act. Gotting. vol. 2. p. 134.

|| Notwithstanding what is said here to shew that sensibility does not infer irritability unless in parts which are muscular, M. de Haller has accused me of inconsistency, when I say that irritability is proportional to sensibility, because I acknowledge the nerves to be destitute of this power. See *Memoires sur les parties sensibles, &c.* vol. 4. p. 118. There is no inconsistency in what I have advanced on this head ; and surely nothing but inadvertency could have been the occasion of M. de Haller's misrepresenting me here.

forth its liquors so copiously, as to separate the scarfskin, and raise it in the form of a bladder filled with water, because the small vessels, of which it is in a great measure composed, partake of a muscular nature, and are, like the larger muscles, excited into alternate contractions by *stimuli*.

FURTHER, the *dartos* or cellular membrane of the *scrotum* is contracted uniformly, when exposed to the cold air or other *stimuli*; and the skin, from the application of cold air or water, seems likewise to undergo some kind of contraction, by which it is raised into tubercles resembling the skin of a goose. When cold water is suddenly and unexpectedly applied to any warm part of the body, the person is instantly seized with a general rigour; and not only the pores of that part to which the cold water was applied, but also of the whole skin, are contracted. Do not these facts shew that the *dartos* and skin are affected by *stimuli*, and consequently are irritable, though not in the same manner with the muscles? The irritability therefore of the parts of the body may perhaps be not improperly distinguished into three kinds: *viz.* That power of alternate contraction which is peculiar to the muscles; that uniform constriction which happens to the *dartos* and pores of the skin; and that redness and inflammation which is excited in every part of the body that is sensible, as often as acrid things are applied; although indeed this last is only an effect of the first kind of irritability in the small vessels of the parts.

I must likewise dissent from M. de Haller in regard to his second argument, *viz.* That irritability is not observed to be in proportion to sensibility; since an inflammation of any irritable organ, which increases its sensibility, is always observed to make it more irritable, as will be shewn afterwards by a variety of instances. The learned author, however, in proof of his assertion, observes, that the stomach is more sensible than the intestines, and yet less irritable; and that the heart itself is endowed with no acute feeling, and, when touched in a living person, occasions fainting rather than pain*.

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* Aët. Gotting. vol. 2. p. 136.

THE stomach has a particular feeling whereby it is offended by things that, as far as we can judge by our taste or smell, have little acrimony: it is the principal seat of hunger; and as, when we have wanted food for any considerable time, it is affected with a more disagreeable sensation than the intestines, so likewise it has more agreeable feelings from grateful food: and in these respects, it may be said to be more sensible than the intestines. But, notwithstanding this, the intestines seem to be as susceptible of pain as the stomach, or indeed, any other organ; an inflammation there is as painful, if not more so, than in the stomach; and jalap, sen-na, and other brisk purgatives, which seldom occasion pain in the stomach, often affect the bowels with severe gripings.

WITH regard to the heart; Harvey seems to have too hastily concluded it to be void of feeling, from that instance which he gives of the young Nobleman whose heart he touched. For what this great man put his finger to, was not the substance of the heart itself, but an insensible *callus* which covered and defended it *. The truth is, that, as the skin, although one of the most sensible parts of the body, feels no pain from a slight pressure or attrition, because it is defended by the insensible *epidermis*; so the heart, when gently touched, feels little, because it is covered with the inner *lamina* of the *pericardium*, which, like other membranes, has but a small degree of sensibility †. In like manner, the external surface of the intestines is rendered less sensible than it would otherwise be, by having a coat from the mesentery; and hence it is, that the woman mentioned by Peyer felt no pain when her intestines were handled by him and Wepfer ‡. But, although the outer surface of the heart:

* Because a testicle covered with a *callus* or fungous substance is sometimes very sensible when touched, M. de Haller seems to think that the small degree of feeling in the heart of that nobleman mentioned by Harvey could not be owing to its callous covering (a). When a part which is covered with *callus* is inflamed, it will doubtless be painful when touched; nay, pressing even the nail gives pain when the nerves below it are inflamed: But to say in general that any part should be rendered more sensible by being surrounded with a *callus*; seems to be too great a paradox.

† Aët. Gotting. vol. 2. p. 130.

‡ Parerg. anatom. exercitat. 1. cap. 4.

(a) Element. physiolog. vol. 1. p. 489.

heart and intestines may have no great degree of sensibility, it will not follow, that their internal surface, where the natural *stimuli* exciting their motions act upon them, is not endowed with a more exquisite feeling: nay, the contrary is highly probable. M. de Haller himself has observed, that the heart is more affected in animals dying, or newly dead, by the gentle *stimulus* of warm water or air pushed into its ventricles, than by applying the most acrid liquors to its external surface, or even pricking it with the point of a penknife *; and it will appear from an experiment to be mentioned afterwards, that, in some cases, the *stimulus* of the blood within the cavities of the heart will excite a tremulous motion there, when oil of vitriol applied to its external surface has no such effect.

WITH regard to the comparative sensibility and irritability of the heart and intestines, it does not appear clearly, from these curious experiments, which of them are the most irritable †. The motions of the heart are indeed stronger and more frequently repeated; but those of the intestines continue, in many animals, as long, if not longer, after death.

As for the third argument, *viz.* That some parts which have no feeling are irritable; there is no experiment offered, and I doubt no instance can be given, of any part being irritable that is naturally insensible and without nerves ‡: but what he thinks equivalent to this, is, that muscles continue to be irritable, not only for some time after their nerves have been tied or cut, and so all communication between them and the brain intercepted, but also after they have been separated from the body. And, indeed, it must be owned, that there is some weight in this argument: but that it nevertheless seems to me inconclusive, has been already suggested in the last section of my Essay on the vital and other involuntary motions of animals; and will, I believe, appear still more so from the following considerations.

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I.

* Aët. Gotting. vol. 1.

† Ibid. vol. 2. p. 147.

‡ M. de Haller indeed mentions, upon the authority of Lupsius, the secundines and membranes of the *ovum* as irritable, tho' without nerves. But, if irritability, as he allows, be a property

1. ALTHOUGH the irritability of muscles continues, in a small degree, for some time after their nerves are tied or otherwise destroyed; it will not follow, as M. de Haller thinks, that this power does not depend upon, or proceed from the nerves: for, if this were so, one would expect that, in a living animal, where the muscles are all supplied with blood by the arteries, they should continue to preserve their power of irritability, not only for a few minutes, but for many hours and days after their nerves have been tied or cut. Further, if the irritability of the muscles were not owing, some how, to the nerves or their influence, why should an irritation of the nerves or *medulla oblongata*, produce such strong convulsions?

THESE convulsions cannot be owing to the propulsion of any subtile fluid in the nerves towards the muscles; since, as M. de Haller and others have observed *, these motions follow equally whether a nerve going to any muscle is squeezed upwards or downwards. If they were owing to the connection or vicinity of the nerves to the muscles, one would expect, that stronger convulsions should follow from the irritation of the tendons than that of the nerves: the contrary of which, however, is true; for, while the irritation of a nerve produces stronger convulsive motions in the muscles, than arise even from the laceration of their own fibres, the tendon, however pricked or irritated, produces no change in them †. The reason is plain; the tendon has little or no feeling, while the nerves have a very acute one.

FURTHER, it ought to be observed, that when, after decollation, the spinal marrow of a frog is destroyed with a red hot wire, no visible motion is produced in its limbs or body, by pricking, cutting, or otherwise hurting them: only, when the skin of the thighs was dissected off, and the muscles were irritated, the fibres of those muscles were agitated with a weak alternate tremulous motion.

Now,

property of muscular fibres alone, it will follow, that the membranes of the *ovum*, which are not muscular, cannot be irritable; but, supposing they were both the one and the other, it is not clear whether they may not be supplied with small nervous filaments propagated to them by the navel string.

* Aët. Gotting. vol. 2. p. 136.

† Ibid. p. 140.

Now, as the strong convulsive motions excited by irritation in the legs and trunk of the body of a frog after decollation, are to be ascribed to the sound state of the spinal marrow, since they cease as soon as it is destroyed; Is it not highly probable, that the weak tremulous motion in the irritated muscles of the thighs of a frog, after the destruction of the spinal marrow, were owing to the influence or power of their nerves, which still remained intire *? It seems also to deserve notice, that, after the destruction of the spinal marrow, altho' the fibres of such muscles as were irritated exhibited a weak tremulous motion; yet there was no sympathy between the different muscles, or other parts of the body, as was observed while the spinal marrow was entire: from whence it seems to follow, that the nerves distributed to the several parts of the body have no communication but at their termination in the brain or spinal marrow; and that to this, perhaps alone, is owing the *consent* or sympathy observed between them.

UPON the whole; the weak alternate motions produced by irritating the muscles, whose nerves have been tied or cut, do not prove, that their irritable power is independent of the nervous influence: they only shew, that these motions are not owing to any new derivation of spirits from the brain into the muscles at that time; that the presence of the nervous influence in their fibres is only requisite; and that the spirits remaining in the nerves, below the ligature and in the muscular fibres, may be sufficient to preserve a certain degree of irritability, or power of motion in them, for some little time.

M. DE HALLER also concludes irritability to be independent of the brain and nerves; because the smallest insects, which have no head, are irritable †. But by the same argument we might prove sensibility and voluntary motion to be also independent of the brain and nerves; for the smallest insects seem to be endowed with feeling,

* As the alternate motions of the heart, in many animals, continue for a long time after the destruction of the brain and spinal marrow; It is not probable, that its nerves are so constituted as to make its moving power less dependent on immediate supplies from the brain and spinal marrow, than that of the voluntary muscles?

† Aët. Gotting. vol. 2. p. 156.

feeling, and undoubtedly perform voluntary motions. May not these insects which want a head have something to supply the place of a brain, from which the nerves may take their rise? Or may not their nerves be so formed, as to be sufficient of themselves, without a brain, for the purposes of motion and sensation?

2. M. DE HALLER, while he denies feeling to the *dura* and *pia mater*, allows it to the medullary substance of the brain*; because, when it is wounded, the muscles of the body are convulsed in an extraordinary manner. Now, if the sensibility of the medullary part of the brain in living animals may be deduced from the convulsive motions which ensue upon hurting it, are we not (the learned author himself being judge) to ascribe feeling to the brain, even in animals newly killed; since in these the motion of the heart is renewed by irritating the *medulla oblongata*, and the whole muscles of the body are convulsed by dissecting the spinal marrow†? And altho', in animals newly dead, the convulsive motions produced by irritating the *medulla oblongata* or *spinalis* be weaker and less remarkable than in living animals; yet it will not follow, that they are not indications of sensibility, and owing to the same cause as in living animals: for, as the death of the body in general soon puts an end to every kind of feeling and activity in the parts of most animals, so it is not to be doubted, that, immediately after death, these powers begin to be weakened; therefore the motions owing to them must be less considerable.

AGAIN, if the convulsions occasioned by irritating a nerve in its natural state are allowed to be a proof of its feeling, the like, though weaker, convulsions excited in the muscles by irritating a cut or tied nerve must be an equal proof of its retaining, in some measure, its sensibility. When all communication, therefore, with the brain, by means of the nerves, is cut off, convulsive motions, which arise from a *stimulus* applied to any part, are as much a proof of the sensibility of that part as if the communication were preserved. And, if in the latter case, these motions may be justly ascribed to the

* Aët. Gotting. vol. 2. p. 130. et 134.; et *Primæ lineæ physiolog.* 2d edit. p. 238.

† Kaau Impet. faciens, No. 330. et 333.

the nerves being hurt by the irritation, they must be equally so in the former.

3. HERE it will be objected; How can there be any sensibility or feeling in a nerve whose communication with the brain is cut off?

IN answer to which, it may be said, that, since we have good reasons for believing that the parts of many insects continue to be sensible for a considerable time after they have been divided from each other *; and that the bodies of some larger animals continue to live and feel after they are deprived of their heads †: Why may we not suppose that the muscles of men preserve some degree of sensibility for a few moments after their nerves are tied or cut, altho' we may not be able to account for this from any thing we know of the nature of the body, or of the manner in which the soul is present with, or acts upon it ‡?

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* Flies copulate and lay eggs after decollation; Boyle's Usefulness of experimental philosophy, part. 2. p. 16.

† Vipers continue for three days after being deprived of their head and heart, to be manifestly sensible of punctures, and move their bodies, when pricked, just as entire vipers do; Boyle's Usefulness of experimental philosophy, part 2. p. 16.

‡ If I were allowed a conjecture concerning a matter of which I know very little, I would say, that, although there can be no feeling or perception in the brain when a nerve is pricked below where it is cut or tied; yet, if the soul be present every where in the body, as seems probable, there may be some kind of feeling or sensation excited in the nerve itself, which may be sufficient to produce a motion in the muscles to which it belongs.

Dr Stewart has produced several arguments to prove, that the inferior extremity of every nerve is to be considered as the brain of the organ or part in which it terminates; and that the soul is not confined to the brain or any part of it, but is present every where in the body, equally in the extremities of the nerves, as at their origin. (Dissert. de motu muscular. cap. 5).

If this be so, as it may for any thing that can be shewn to the contrary, why may not a muscle, whose nerve is tied or cut, continue, for some little time, sensible and irritable? Its sensibility will not indeed be attended with what is called consciousness, as distinguished from simple sensation; because this reflex act, by which a person knows his thoughts or sensations to be his own, is a faculty of the soul exercised in the brain only, with which all communication is now cut off.

As the soul seems to imagine, judge, reason, and remember in the brain only; why may it not have, in the various other parts of the body, such feelings or powers as are necessary for carrying on their several functions? In particular, why may it not have, in the muscular fibres, the power of simple sensation and of beginning motion? Or, which will amount to much

REDI informs us, that the head of a viper will bite half an hour after it is cut off from the body, (*Vid. Jacobæi Observat. de ranis et lacert. p. 58. ;*) and I have often observed, that the head of a frog, after being separated from the body, not only continued, for above half an hour, to move the eye-lids, nostrils, and muscles of the lower jaw, when the brain or the skin of the head was touched with a probe, but sometimes moved the eyes and eye-lids, when nothing touched it, and as it were of its own accord; so that, without too much scepticism, we cannot deny that the head continues to be animated for some time after it is separated from the body, and to perform

much the same, while the rational soul acts only in the brain, there may perhaps be, as some have thought, a sentient active principle, which enlivens the whole body, and which continues to actuate the parts for some time after their communication with the brain is stopt, *i. e.* as long as they continue in due order for being acted upon by it.

The more probable opinion, however, seems to be, that the soul is equally present in the extremities of the nerves through the whole body as in the brain. In those, it is only capable of feeling or simple sensation; but in this, it exercises the powers of reflex consciousness and reason. When the communication of any part with the brain is cut off, the simple sensation of feeling excited in such part is no longer perceived by the soul in the brain; and therefore is not attended with reflex consciousness: the nerves being then also separated from the brain, soon become unfit to perform their functions; hence the powers of simple sensation and motion in the part, if it be muscular, cease by degrees, till at last it becomes quite dead. The communication, therefore, between the several organs and the brain, is not only necessary to preserve their nerves, by means of some influence transmitted to them in due order for performing their functions and being properly affected by their several objects, but also, that the soul, as a conscious and rational being, may be acquainted with these impressions.

It cannot well be objected here, That we ascribe the intelligent powers of the mind to the bodily organs: for as the best musician cannot make a flute give the sound of a violin, nor a harpsicord that of a French horn, nor without those several instruments produce their sounds and notes at all; in like manner, the soul, in the present state, can only exercise its rational powers in the brain; it can only taste in the tongue, smell in the nose, see in the eyes, hear in the ears, and feel hunger in the stomach. But although the imagination, memory, and rational powers, depend upon the brain; yet the brain does not imagine, remember, nor reason: although taste depends on the tongue, smelling on the nose, seeing on the eyes, and hearing on the ears; yet those organs neither taste, smell, see, nor hear, but only that living sentient principle which animates them.

It may be proper to observe, that, whether these conjectures, which are offered with much diffidence, shall be thought probable or not, the argument concerning the irritable power of the muscles of animals will not be materially affected; since this must be determined, not by metaphysical reasonings, but by experiments and observations. *Vid. sect. 4. below.*

perform not only involuntary motions when stimulated, but, in appearance, also voluntary ones. In like manner, the body of a frog, after being divided from the head, preserves the power of motion for above an hour; and when its hind feet or toes are cut, or otherwise hurt, the muscles of its thighs, legs, and trunk are strongly contracted, by which it raises its body from the table, and sometimes moves from one place to another. When the muscles of the thighs are pricked or cut with a knife, they are excited into contraction; but neither they, nor the neighbouring muscles, are near so strongly convulsed as when the toes are wounded. Whence should this happen; and why should not the muscles of the legs and thighs be more strongly convulsed, when they themselves are wounded, than when the toes are treated in the same manner? This would be the case, if the motions of irritated muscles were owing to some property of the insensible matter composing them. But if, as we imagine, they are all to be derived from feeling, it is easy to see, that, as the feet and toes are more sensible of pain when wounded, than the muscles of the legs or thighs, stronger convulsions must be occasioned by an irritation of the former than of the latter.

FURTHER, we must either allow that both the head and body of a frog continue to be animated for some time after they are separated from each other; or else affirm, that the life, feeling, and active powers of animals, are merely properties of that kind of matter of which they are made. The former opinion is attended with some difficulties, which arise chiefly from our ignorance of the nature of immaterial beings: the latter seems to be inconsistent with all the known properties of matter. If the latter therefore be admitted, we not only ascribe qualities to matter which it does not possess, but presume to limit, by our narrow and inadequate capacities, the powers of incorporeal natures, their manner of acting upon bodies, and co-existing with them.

If the soul were confined to the brain, as many have believed *, whence is it that a pigeon not only lives for several hours after being deprived of its brain, but also flies from one place to another †?

O o

And

* Aët. Gotting. vol. 2. p. 153.

† Baghivi opera, 4to, prefat. p. 11.

And to what cause are we to ascribe the continuance of life and motion in a viper for three days after its head is cut off, and in a tortoise for three weeks after decollation, and six months after the loss of its brain * ? The motions performed by these animals cannot surely be attributed to their material part alone; unless we shall deny them a soul altogether, and, with Des Cartes, refer all their actions to their corporeal machinery. The late Reverend and learned Dr Hales informed me, that having many years since tied a ligature about the neck of a frog to prevent any effusion of blood, he cut off its head, and, thirty hours after, observed the blood circulating freely in the web of the foot: the frog also at this time moved its body when stimulated: but that, on thrusting a needle down the spinal marrow, the animal was strongly convulsed, and immediately after became motionless.

If then the soul in pigeons, frogs, vipers, and tortoises, is not confined to the brain, but can continue for a long time to actuate their bodies independent of that organ; and if, in many insects which have no brain, every part of the body is both sensible and irritable †; why should we deny, that, in man and such animals as resemble him most, the parts may continue to be actuated by the soul or sentient principle for some few minutes after their communication with the brain has been cut off ‡?

If any man of ordinary sense, who is no philosopher, be asked, Why the heart of a frog beats after being separated from the body, and renews its motions when pricked? he will readily say, because there is life in it. This is a proper answer; for a better, perhaps, cannot be given by the ablest philosopher. If then life in animals be owing to the energy of a principle distinct from matter, and of powers superior to it, we have reason to conclude, that, as long as any

* Redi. Observat. circa animal. vivent. p. 209. &c.

† Act. Gotting. vol. 2 p. 138.

‡ The difference between men and those animals which live long after decollation or the excision of their heart, seems to be, that the latter are so framed that fresh supplies of blood and spirits from the heart and brain are not immediately necessary to keep the several parts in due order to be acted upon by the soul: as seems to be, in a great measure, the case in man and many other animals.

any signs of life remain in the bodies of animals, or in any of their parts, this principle still continues to actuate them.

THERE are two kinds of motion from irritation observable in living animals, *viz.* where the muscle or organ itself is stimulated; and where the *stimulus* only affects some neighbouring or distant part. The first (of which kind is the motion of the heart) seems to be owing to the soul or sentient principle as acting in the part moved; but the second, to the soul as perceiving and acting in the brain: and of this kind is the motion of sneezing from an irritation of the nose, and the contraction of the diaphragm in vomiting and in a *tenesmus* or stranguary. In order to the first kind of motions, an immediate communication with the brain is not absolutely necessary, but only such a share of the nervous power in the muscle or its nerves, as may be requisite to fit its fibres for being acted upon by the soul or sentient principle. But the case is quite otherwise in the second; where the motion produced is thro' the intervention of the brain, and not by any *stimulus* applied to the part moved. And hence it is, that, in an animal newly dead, the diaphragm is not brought into contraction by lacerating or pricking the *intestinum rectum* or neck of the bladder, although the fibres of these parts themselves may be thereby agitated with some tremulous motions. In like manner, though the muscular coat of the stomach is excited into contraction some time after the death of an animal by irritating it; yet the diaphragm is not affected by this irritation: which, however, it would have been, if the animal had been alive. Agreeably to this, when any of the muscles of the legs of a frog are irritated some time after cutting off its head, almost all the muscles belonging to the legs and thighs are brought into contraction, if the spinal marrow be entire: but, as soon as it is destroyed, although the fibres of such muscles as are themselves stimulated are affected with a weak tremulous motion, yet the neighbouring muscles remain at perfect rest.

I have elsewhere endeavoured to shew, that the supposition of the soul or sentient principle's continuing for some time to actuate

the separated parts of animals, does not infer its *real* divisibility*; nor is it necessary to repeat the same things again: but I cannot help observing, that, when M. de Haller represents me as holding the soul to be divisible, so as that it may be cut into as many pieces as the anatomist pleases †, he inadvertently charges me with an opinion which I not only do not maintain, but which I have brought arguments to disprove. I shall only add, that the indivisibility of the soul does not depend on the unity of the body, but on its own particular nature.

It must be acknowledged, that there is a great deal of obscurity in these matters: but as in every part of nature we find abundance of mysteries, as often as we carry our inquiries to any great length; it can be no wonder if we meet with difficulties, almost insurmountable, in accounting for the motions of animals, or tracing them up to their source: for, if we are far from understanding the communication of motion and other actions of matter upon matter, how shall we be able to comprehend the manner in which an immaterial principle acts upon it? But, as we can, from the little we know of matter, see that inactivity is one of its essential properties, we are hence convinced of the necessity of ascribing the life and motions of animals to the power of an incorporeal agent.

S E C T. III.

THE learned M. de Haller, after endeavouring to prove that irritability is independent on sensibility, gives it as his opinion, that this remarkable property of the muscles has its seat in the glutinous matter connecting the earthy elements of which their fibres are composed ‡; and that irritability ought to be considered as a peculiar property of this glutinous substance, in like manner as gravity is allowed to be a property of matter in general, altho' its cause cannot be assigned ||.

BUT

* Essay on vital motions, &c. p. 183. &c.

† Act. Gotting. vol. 2. p. 137.

‡ Act. Gotting. vol. 2. p. 152.

|| Ibid. p. 154. & 157.

BUT surely the glutinous matter of the muscles of animals seems as unlikely to be endowed with an active power, such as irritability, as any other constituent part of the animal body; nor can any argument be deduced from its endeavouring to shrink or shorten itself when drawn out*; for the glue of the skin, ligaments, and tendons, as well as of the muscles, has this property, which is, indeed, a kind of elasticity†, and not similar to that power of alternate contraction which muscular fibres are endowed with.

IN proof of his notion of the irritable nature of the muscular glue, he adds, that young animals which abound most with it are most irritable. The observation is true, but seems to prove nothing in the present case; for the skin, ligaments, and tendons (which last are a continuation of the muscles, only harder and more compacted) abound more in glue than the muscles, and yet are in no degree irritable. The greater irritability of the fibres of young animals is to be deduced from their greater sensibility, and this is owing to their greater softness and tenderness: thus, what in new-born animals is a sensible and irritable muscle, becomes afterwards a tendon, which, in a sound state, is free from irritability, and is endowed with little or no feeling‡.

FURTHER, since the gelatinous matter in our aliments, and even in our blood, is quite without any irritability, it must owe this power to the particular disposition or arrangement of its parts, or to some other change which it undergoes when it becomes a part of a muscle. If this may be so, why may not the finer and more subtile parts of the blood be so changed in the brain, as there to acquire a power of feeling and thinking? *i. e.* if irritability be a property of the muscular glue, why may not sensibility and intelligence be properties of the medullary substance of the brain? for the known properties of matter give us reason to think, that real activity is not more consistent with its nature, than feeling or thought.

BUT

* Aët. Gotting. vol. 2. p. 152.

† Elasticity is not a property of hard bodies alone, as M. de Haller seems to think (p. 152.), but is also found in soft ones: thus air, wool, and the down of feathers are remarkably elastic.

‡ Aët. Gotting. vol. 2. p. 140.

BUT it has been said, that irritability may be a property of the muscular glue, as well as gravity is a property of matter in general: let us therefore consider this notion a little, and see whither it will lead us. Gravity, which is a property of matter, continues to be so, let matter be ever so much altered or changed by fire, menstruums, or other causes; but, when the *gluten* of the muscles is extracted from them, it appears as inert and void of active powers as any other matter; nay, though allowed to remain in them, yet, in most animals, it wholly loses its irritable power soon after the muscles are separated from the body.

BUT, supposing irritability to be a property of the muscular glue, in the same sense that gravity is a property of all matter; yet, as the most attentive consideration of matter has convinced philosophers that gravity is not essential to it, but owing to some general cause acting upon it; so the irritability of the muscular glue must be allowed not to be a property essential to it, but arising from the action of some other cause upon it. Gravity has been ascribed either to the immediate and continued operation of an immaterial being, or to the action of some subtile elastic medium on matter: but, since the elasticity of the parts composing such a medium must be, at last, referred to the active power of some incorporeal cause, it follows, that gravity must be so likewise*.

IT appears, therefore, after all that has been said to shew that the motions of irritated muscles are owing to a property of irritability in them or their glue, that we are at last obliged to refer them to the active power of an immaterial cause; unless we shall, contrary to sound philosophy, ascribe feeling and spontaneous activity

* M. de Haller has considered me as maintaining an opinion contrary to the common notions of mankind (*a*), when I say, that gravity, or rather the elasticity of that medium supposed to be the cause of gravity, must be, at last, referred to the active power of some incorporeal cause. How far we should be directed in our reasonings concerning the nature or cause of gravity by the common notions of mankind, I shall leave the reader to determine; but some able philosophers have thought, and not without reason, that gravity, or the material cause producing it, must at last depend upon the power of that BEING who sustains, moves, and governs the whole system of nature: and M. de Haller will find it a hard task to account for the elasticity of that æthereal medium supposed to be the cause of gravity, from any thing we know of the nature of matter.

(*a*) Memoires sur les parties sensibles, &c. vol. 4. p. 91.

vity to matter. And, as gravity must finally be resolved into the power of that BEING who upholds universal nature; so it is probable, that the irritability of the muscles of animals is owing to that living sentient principle which animates and enlivens their whole frame.

S E C T. IV.

HAVING thus proposed my difficulties concerning the learned M. de Haller's ingenious theory of irritability, I shall conclude with a few observations, which, if they do not demonstrate, make it at least probable, that the motions of stimulated muscles proceed from their sensibility, or that they are closely connected with it. But, previous to these, I must be allowed to take notice, that the word *irritability* seems to imply a kind of life or feeling in the part endowed with it, which renders it capable of being fretted, provoked, or irritated; and therefore seems to be improperly applied to express the contractile power of stimulated muscles, if this power has no connection with, or dependence on their sensibility *. We never talk of irritating a stone, a piece of wood, a tree, or indeed any thing that is without feeling. Irritability, therefore, in the common acceptation of the word, implies some kind of feeling; nay, M. de Haller himself, notwithstanding that his professed design seems to be to shew irritability to be independent of sensibility, yet once and again speaks of parts that are not irritable, as not feeling or perceiving the acrid matter, or other *stimulus* applied to them †.

BUT to return:

I. WE almost always observe the irritability of the muscles or muscular organs of the human body to bear a proportion to their sensibility.

* "Quicquid incommodum alicunde illatum percipit, idemque a se amoliri satagit, id proprie dixeris irritari. Idcirco, cum partes istæ injurias persentiscant, seseque ab iisdem vindicare conentur, irritationis quoque capaces merito dicendæ sunt;" Glisson Anatom. hepat.

† Acta Gottingenf. vol. 2. p. 142.

fenfibility. Thus, in young children, where the tender nerves and fibres are more easily hurt, and all the feelings are more exquisite; the quickness of the pulse and the violent convulsions with which they are often affected, from flight causes, shew their muscles to be impressed with a greater degree of irritability than those of adults *. In like manner, grown people of delicate nerves and very quick feelings are subject to spasms and convulsive motions of their stomach, intestines, &c. and to palpitations of their heart, from such flight causes as would scarce sensibly affect men of firmer constitutions and less moveable nerves.

ON the other hand, in old people, in whom all the feelings become less acute, the muscles are less irritable; witness the slow motion of their heart. And, in apoplectic and comatous cases, where the senses are greatly impaired, the motion of the heart and that of respiration are remarkably slow; and the *stimulus* of the *feces* is not sufficient to bring the intestines, diaphragm, and abdominal muscles into contraction as usual.

AGAIN the nerves, which are the most sensible parts of the body, produce, when irritated, the strongest convulsive motions in the muscles; and, when they are, by being stretched, rendered more susceptible of pain, an irritation of them produces still greater convulsions †.

II. WHATEVER increases the sensibility of the muscles or moving organs of our body, also increases their irritability.

THUS, when the stomach is inflamed, the mildest liquors are apt to provoke vomiting, or the hiccup; whereas, in the sound state of that organ, brandy, vinegar, and other acrid liquors, produce no such effect. When the neck of the bladder is slightly inflamed or excoriated, the urine, which used to give little disturbance till collected in large quantity, irritates that tender part so as to occasion violent and often repeated efforts to empty the bladder.

WHEN

* It may also be observed, that the parts of young animals which are most sensible, are not only most irritable, but retain their power of motion longest after death, or separation from the body.

† Act. Gottingenf. vol. 2. p. 136.

WHEN the *fauces* are inflamed, the muscles of deglutition are more strongly convulsed in swallowing than when those parts are in their natural state. When the intestines are deprived, in a good measure, of their *mucus*, or become more sensible by a slight degree of inflammation in their inner membrane, the mildest purgatives will often operate as roughly as the stronger ones do in a person in perfect health. When, without any erection in the *penis*, the *semen* escapes into the *urethra*, the *musculi acceleratores urinæ* are not affected by it: but, as often as the *penis* is erected, whereby its parts become more sensible, and, as it were, half inflamed, the *semen* is no sooner thrown into the beginning of the *urethra*, than those muscles are excited into strong convulsive contractions.

THE heart becomes so irritable, when either itself or the *pericardium* is inflamed, as to be agitated with violent convulsions and palpitations. Nay, the tendons, which, in a sound state, have little or no feeling, and are not irritable *, become, when inflamed, so sensible of *stimuli*, that the strongest convulsions have been occasioned by pricking, tearing, or otherwise irritating them.

A disagreeable sensation in the stomach from wind and other causes, often quickens (especially in people whose nervous system is delicate and moveable) the motion of the heart; which will often return to its natural motion by a glass of wine, or spirits, or any thing that, by invigorating the stomach, removes its uneasy sensation.

A disagreeable feeling in the stomach makes the heart more irritable, because, by means of its nervous sympathy with that organ, it increases its sensibility; and, in like manner, an inflammation or an unusual irritation in the kidneys or intestines increases the irritability of the stomach: but how a disagreeable feeling in the stomach should immediately alter the nature of the *gluten* of the fibres of the heart, I cannot conceive.

If therefore it appears, that the irritability of the moving organs of our body is increased as often as their own sensibility, or that of other parts with which they have an observable sympathy, is in-

P p

creased;

* Aët. Gotting. vol. 2. p. 140.

creased; it will be thought probable, that the irritability of any part depends upon its sensibility.

III. WHATEVER lessens or destroys the sensibility of the muscles of animals, also lessens or destroys their irritability or power of motion.

THUS, when one's fingers or limbs have been long exposed to severe cold, they not only become insensible, but paralytic. Frogs, bats, and other animals, with numbers of the insect-tribe, are so benumbed by the winter's cold as to be deprived of all feeling and motion: their blood does not circulate, nor their hearts beat; and their muscles, though torn, cut in pieces, or otherwise stimulated, are not brought into contraction.

DURING the time of incubation, the heart of the chick is observed to beat faster or slower, and with more or less force, *i. e.* to become more or less irritable, as it is exposed to greater or less degrees of heat; nay, after its motion has been wholly stopt by cold, a gentle heat will make it soon begin to contract a-new*.

FURTHER, this *punctum saliens*, or heart of the chick, which, when touched with any thing capable of hurting it, is excited into quicker and stronger contractions, after being exposed for some time to too great cold, is not affected by the most powerful *stimuli*.

IT appears, therefore, that feeling and irritability are destroyed by cold, and restored by a proper degree of heat, and are so closely connected together, that the latter is never to be found where the former is totally wanting.

IF authority could be of any weight in a matter which is to be determined by experiments and observations, we might support our opinion with the name of one of the most judicious and successful inquirers into nature. "Ego pluribus experimentis certus sum, (says the illustrious Harvey), non motum solummodo puncto salienti inesse, sed sensum etiam; nam, ad quemlibet, vel minimum, tactum, videbis punctum hoc varie commoveri, et quasi irritari. Vidi, inquam, sæpiissime, alique qui una mecum ade-

"rant,

* Harvey De generat. animal. exercit. 17.

“ rant, ab acus, styli, aut digiti contactu, immo vero a calore aut
 “ frigore vehementiore admoto, aut cujuslibet rei molestantis oc-
 “ cursu, punctum hoc varia sensus indicia, pulsuum nempe varias
 “ permutationes, ictusque validiores ac frequentiores, edidisse; ut
 “ non dubitandum sit, quin punctum hoc (animalis instar) vivat,
 “ moveatur, ac sentiat.” *De generat. animal. exercit. 17.*

OPiUM, which is remarkable for its power of impairing or destroying the sensibility of all the parts of the body, also lessens or suspends the irritability or moving power of the muscles. Thus, in a small dose, it puts a stop to vomiting and coughing; and quiets the convulsive motions of the *intestinum rectum*, bladder, abdominal muscles, and diaphragm, in a *tenesmus* and stranguary, although the *stimuli* which produced these motions continue to act on the parts: when given in much larger quantity, it suspends the peristaltic motion of the intestines, and makes the heart contract more slowly, till being by degrees rendered quite insensible, its motion wholly ceases.

BUT, as my learned antagonist, who allows that *opium* destroys the irritability of the stomach, intestines, and other muscles, denies it to have any power over the heart, * and seems to call in question those experiments of mine which shew that *opium*, injected into the stomach and intestines of frogs, renders the motion of the heart much slower than usual, and at last puts a final stop to it †; I thought it necessary to endeavour to clear up this matter by some farther experiments, which I shall here briefly relate.

(a) JUNE 5. 1755, at 18 minutes past four in the afternoon, I injected a turbid solution of half an ounce of *opium* in eight ounces of water, into the stomach and intestines of a frog; and, as it squirted out most of the solution injected by the *anus*, I threw in some more in its room. At 24 minutes past five the same evening, I opened this frog, and observed the heart beating very slowly, not above seven times in a minute; when it was touched with the point of a pair of scissars, it renewed its motion faster for two or three

p p 20 who says: pulsations;

* Act. Gotting. vol. 2. p. 147, 154, and 157.

† Essay on vital motions, p. 197. &c.

pulsations ; after which it became as slow as before. The other muscles of this frog were not at this time brought into contraction by pricking or tearing their fibres.

(*β*) I laid open the whole *abdomen* and *thorax* of a frog ; and, at 28 minutes past seven in the morning, immersed it in a turbid solution of *opium*, viz. the same that was made use of in the preceding and following experiments. At forty minutes after seven, I turned the frog upon its back, and observed its heart beating between ten and eleven times in a minute. Having laid it again upon its belly, that it might be more exposed to the action of the *opium*, at forty eight minutes past seven, I turned it again upon its back, and observing the heart without motion, I opened the *pericardium* ; which producing no effect, I cut the heart out of the body, and laid it upon a plate, when it beat twice or thrice, and never after moved, although it was pricked once and again with a pin.

(*γ*) I cut off the head of a frog, and intirely destroyed its spinal marrow by pushing a small probe down the spine, which occasioned strong convulsions of all the muscles, especially those of the inferior extremities. Ten minutes after this, I opened the *thorax*, and found the heart beating 45 times in a minute. Sixteen minutes after decollation and the destruction of the spinal marrow, it moved 40 times in a minute. After half an hour, it made 36, and, after fifty minutes, only 30 pulsations in the minute, which were now become very small and feeble.

WHEN the *thorax* of another frog was opened immediately after decollation and the destruction of its spinal marrow, its heart beat 60 times in a minute.

(*δ*) I cut out the heart of a frog, and put it into spring-water, at twenty-three minutes past twelve. After twelve minutes immersion, I took it out of the water, when it beat 20 times in a minute. Having immersed it for five minutes more, it ceased from motion ; and when taken out of the water, it did not move except when pricked, and then it gave only one pulsation.

(*ε*) EIGHT minutes past eleven, I cut out the heart of another frog, and

and immersed it in spring-water. Twenty-eight minutes after eleven, it continued to move: but its motion, tho' at the rate of eleven pulsations in thirty seconds, was confined to about one third of the heart next its *apex*. Two minutes after this, observing it without any motion, I took it out of the water, and laid it upon a table, where it remained at rest, unless when touched. Soon after this, however, it began to move, and, at 25 minutes after immersion, performed nine pulsations in 63 seconds.

(1) I cut out the heart of a frog, and, at 32 minutes after ten, immersed it in a turbid solution of *opium* made in water of the same degree of heat with the spring-water used in the two last experiments *. After it had been immersed ten minutes, I took it out of the solution, and laid it on a table; but then it had not the smallest motion: and, when pricked with the point of a penknife, tho' it quickly recovered its shape, yet it was not excited into a proper contraction like the heart of †. I continued to observe this heart from time to time for above half an hour, but it never moved.

(2) I cut out the heart of another frog, and put it into the same turbid solution of *opium*; after seven minutes immersion, I took it out and laid it on a plate, where it remained at rest. When pricked with the point of a penknife, it did not make a full pulsation, but seemed to feel a little, by a very faint kind of motion which I perceived in some of its fibres.

(3) MR Robert Ramsay student in medicine ‡, at my desire, made the following experiment. After making an opening into the cavity of the *abdomen* of a small dog near six months old, he injected by the wound a dram of *opium* dissolved in two ounces and a half of water; but, before he could stitch up the wound, about an ounce of the solution escaped. Four minutes after the injection, he laid bare the *thorax*, by dissecting off the teguments, which seemed to give the animal no pain; and could plainly feel the motion of his heart thro' the *pleura*. It beat 76 times in a minute, but became gradually slower §. Immediately after counting the pulse, Mr Ramsay

* Viz. nearly 60 degrees of Fahrenheit's thermometer.

† Now Fellow of the Royal College of Physicians in Edinburgh.

‡ The heart of this dog before the injection of the *opium* beat 150 in a minute.

say cut the ribs on each side of the *sternum*, which he laid back in the usual way. The heart, which was thus brought into view, appeared turgid, and continued in motion about five minutes: during which time it performed only between 60 and 65 weak vibrations; for they were not full contractions. While the heart was thus moving, some warm *saliva* was first applied to it, then cold water, and, last of all, oil of vitriol, which shrivelled the parts it touched almost in the same manner as a hot iron would have done; but none of them accelerated the vibrations of the heart, which became gradually slower, till they intirely ceased.

AGREEABLY to this experiment, we are informed by Dr Alston, in his learned dissertation on *opium*, that a filtrated solution of this medicine in water having been injected into the veins of a dog, his pulse, which, when he was first seized with convulsions, was rendered quick and small, became afterwards full and slow*. And Dr Kaau Boerhaave observes, that in a small dog, which he opened ten hours after he had swallowed three grains of *opium*, the motion of the heart and arteries was very slow†.

FROM these experiments it appears, that as *opium* destroys the sensibility of all the parts of the body, so it deprives the muscles of their power of motion; nor is the heart in this respect possessed of any privilege above the other muscles, except that its moving power is not so soon destroyed by *opium* as theirs.

How M. de Haller was deceived in this matter, I cannot conjecture; since he has not told us in what manner his experiments were made: but, it is not to be doubted, that from his candor and love of truth he will readily acknowledge his mistake, as soon as he shall discover it.

IV. WHEN a viper is pricked with the point of a knife three days after being deprived of its head, heart, and other *viscera*, it moves, not only those muscles whose fibres are touched, but also the other muscles of its body which have no connection with those that are stimulated. This indicates either a sympathy between those muscles,

* Vid. Medical Essays, vol. 5. p. 1. art. 12.

† Cor lentissime movebatur. Motus in arteriis (scil. duræ et piz matris) debilis et valde lentus. Vid. Impet. faciens Hippocrati dictum. No. 434. et 436.

muscles, which supposes feeling, or some general active principle animating them, which, being affected with a disagreeable sensation by the *stimulus* applied to any one muscle, brings many others into action, in order to avoid what is hurtful to it. In like manner, when a few drops of hot water fall upon one's leg, the muscles which move it are instantly and involuntarily brought into contraction, in order to remove it from the offending cause.

A frog, after it has been deprived of its head, when touched, often jumps and moves about for a considerable time; and it is observable, that, when the toes of its hind feet are any way stimulated, it draws the feet up to its body; nay, when they are in this situation, if the toes are again irritated, the legs and feet are not extended, but brought still closer to the body. If one of the legs is pulled down from the body, and kept extended, no sooner are the toes of this foot wounded than the leg is drawn up to the body as before. Now, if these motions were owing to some property of the insensible matter of which the muscles consist, why should not an irritation of the toes be sometimes followed by a contraction of the *extensor* as well as the *flexor* muscles of the legs and thighs? But, if we allow them to be owing to the painful sensation in the toes, we shall see that the frog does, in this case, with its limbs, just what a snail does with its horns, when they are roughly touched.

AGAIN, it is observable, that, when the toes of a frog are pricked or otherwise wounded instantly after decollation, there is either no motion produced in the muscles of the legs at all, or a very inconsiderable one. But, if the toes of a frog be touched with one's finger ten, fifteen, or twenty minutes after decollation, the legs and thighs are immediately drawn up to its body; and if at this time they be wounded, pricked, or cut with a penknife, the muscles, not only of the legs and thighs, but also of the trunk of the body, are, for the most part, strongly contracted, and the animal sometimes moves from one place to another.

Is not the irritation of the toes, immediately after decollation, rendered ineffectual for producing any motion in the muscles of the legs and thighs, by the greater pain occasioned by cutting off

off the head *? And are not the muscles of the posterior extremities, as well as those of the trunk of the body, brought into action by wounding the toes fifteen or twenty minutes after decollation, because the pain occasioned by cutting off the head is now so much lessened (perhaps wholly obliterated) as not to prevent the animal from feeling very sensibly when its toes are hurt?

V. THAT the motions of irritated muscles are owing to the sensation excited by the *stimulus* applied to them, will appear probable, if we consider, that we are in fact conscious of many involuntary motions in our bodies proceeding from a particular sensation, either in the organs moved, or in some neighbouring part. This is the case with the motions of the stomach and diaphragm in vomiting and in the hiccup, of the great guts and diaphragm in a *teneismus*, of the *acceleratores urinae* in expelling the *semen*, and of the intercostal muscles and diaphragm in sneezing, coughing, and sometimes even in breathing; nay, when by sudden fear or any great surprise the heart is set a palpitating, we have a peculiar feeling in this muscle, partly from the blood rushing suddenly and in too great quantity into it. More examples might be given; but these may suffice to shew the connection there is betwixt the sensibility and irritability of the moving organs of our body.

UPON supposition that the motions of irritated muscles did not proceed from any kind of feeling, but from some inanimate cause, their contractions should be all, either regularly alternate, or equable and uninterrupted, like the closing of the leaves of the sensitive plant †; but we find, that, while most of our muscles are brought by the action of *stimuli* into alternate contractions, there are some few which contract uniformly and equably during the time the *stimulus* operates, without any intermissions or alternate relaxations. Of this kind is the contraction of the diaphragm and abdominal muscles when

* Duobus doloribus simul obortis, non in eodem loco, vehementior obscurat alterum. Hippocrat. Aphor. lib. 2. No. 46.

† I have elsewhere shewn by experiments, that the closing of the leaves of the sensitive plant, when touched, does not indicate any kind of feeling, and is no way similar to the alternate contractions of irritated muscles. Essay on vital motions, &c. p. 130.

when the *intestinum rectum* is irritated, of the *sphincter pupillæ*, while the same degree of light continues to act on the *retina*, and of the muscles of the internal ear as long as the same sound is applied to that organ. Nay, the diaphragm, which is brought into one continued contraction by a *stimulus* affecting the *intestinum rectum*, is agitated with alternate convulsions from an irritation of the left orifice of the stomach, or of the olfactory nerves. What account can be given of this, upon supposition that these motions proceed from the *gluten* of the muscular fibres? or what difference can it make to this insensible *glue*, whether the *stimulus* be applied to the nose or *anus*? But, allowing these motions to arise in consequence of an uneasy sensation in the part stimulated, it will appear, that they are performed in such a manner as is most effectual to lessen or remove the irritating cause *.

AGAIN, if the motions of muscles from *stimuli* were not owing to a feeling, how could the convulsive motions of the diaphragm in the hiccup be often immediately stopped by sudden fear, joy, or grief? Why should an irritation of the olfactory nerves become ineffectual to produce sneezing, when some of the muscles of the back or *thorax* are affected with a rheumatism? And why should the convulsive motions of the stomach and diaphragm in vomiting, be frequently interrupted by extraordinary fear, or any great and sudden surprise? It will be hard to give any satisfactory solution of these appearances, if the motions of irritated muscles are supposed to proceed from some unknown property of their *insensible glue*: whilst they may be naturally accounted for upon the principle that they are owing to an uneasy sensation; for as often as this feeling is overpowered by a stronger one in some other part of the body, or when the mind is so suddenly and strongly affected by external objects, as, for a short time, to become almost insensible of the irritation, the motions owing to it must be lessened or cease.

GRAVITY, magnetism, and electricity, are all regular and uniform in their operations; they bespeak nothing of feeling or life in

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* Vid. An Essay on the vital and involuntary motions, p. 138.

the bodies which are endowed with them, and may therefore be supposed to proceed immediately from material causes ; although the activity of these causes must be, at last, referred to the great ORIGIN of all power and life in the universe. But the motions of animal bodies from a *stimulus* are, in many cases, so plainly perceived to flow from an uneasy feeling, their various appearances can be so easily explained upon this supposition, and are so unaccountable on any other, that I am surpris'd to find so many learned and ingenious physiologists endeavouring to refute this opinion, and to derive those motions from inanimate matter.

LIFE, sense, and self-activity, seem to be inconsistent with the known properties of matter ; and therefore, when we see a system of matter endowed with these, we may, without presumption, conclude, that they are owing, not to the material system alone, but to some active principle animating it. And although, even upon this supposition, it may be difficult to account for some of the motions observed in such a system, or in its parts when separated, we cannot therefore conclude, that they are not owing to any such power ; but only that our ignorance of the nature of immaterial beings, and of their union with, and manner of acting upon bodies, throws a veil of obscurity over those things which the most enlightened philosopher will never be able to remove.

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Some EXPERIMENTS made with OPIUM on Li
ving and Dying ANIMALS.

First published

In the Edinburgh PHYSICAL and LITERARY ESSAYS.

AN
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Some EXPERIMENTS made with OPIUM on Living and
Dying ANIMALS.

THE ancient physicians imagined that *opium* extinguished the flame of life in animals by its excessive cold; and in later times, there have not been wanting those who deduced its effects from a quite opposite quality, whereby it was thought to rarify the blood and to compress the brain or origin of the nerves. These false notions, however, of the nature and action of *opium*, have been refuted by several of the moderns, whose writings have thrown considerable light upon this subject.

THE following experiments were made with a view still further to illustrate the manner in which this wonderful drug produces its effects, and particularly to shew its influence upon the motion of the heart.

1. HAVING injected a solution of *opium* in water into the stomach and guts of a frog, I observed, that in little more than half an hour it seemed to have lost all power of motion, as well as feeling; for there was no contraction produced in the muscles of its limbs and trunk by irritating them. I opened the *thorax* an hour after the injection, and found the heart, instead of between 60 and 70, making only 17 pulsations in a minute. The auricle, which was much distended with blood, always contracted first, and after it the ventricle.

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2. A frog continued to move its limbs, and leap about for above an hour after I had cut out its heart, and was not quite dead after two hours and a half.

FIVE minutes after taking out the heart of another frog, I injected a solution of *opium* into its stomach and guts. In less than half an hour, it seemed to be quite dead; for neither pricking nor tearing its muscles produced any contraction in them, or any motion in the members to which they belonged. After cutting off its head, a probe pushed into the spinal marrow made its fore-legs contract feebly.

3. EIGHTEEN minutes past four in the afternoon, I injected a stronger turbid solution of *opium* in water than that used in the preceeding experiments *, into the stomach and guts of a frog; and as it squirted out most of the solution injected by the *anus*, I threw in some more in its place. At twenty-four minutes past five, I opened this frog, and observed the heart with its auricle greatly distended with blood and beating very slowly, not above seven times in a minute. When the heart was touched with the point of a pair of scissars, its motion was rendered quicker for two or three pulsations: after which it became as slow as before.

4. IMMEDIATELY after decollating a frog, I destroyed its spinal marrow, by pushing a small probe down through its spine, which occasioned strong convulsions of all the muscles, especially those of the inferior extremities. Ten minutes after this, I opened the *thorax*, and found the heart beating at the rate of 45 times in a minute. Sixteen minutes after decollation, it moved 40 times in a minute. After half an hour it made 36, and after fifty minutes only 30 pulsations in the minute, which were now also become very small and feeble.

N. B. WHEN I opened the *thorax* of another frog immediately after decollation and destroying its spinal marrow, I observed its heart beating at the rate of 60 in a minute, which is four or five pulsations

* Viz. half an ounce of *opium* dissolved in eight ounces of water; which was also made use of in all the following experiments. The heat of the solution was nearly the same in all the experiments; viz. about 60 degrees of Fahrenheit's thermometer.

pulsations less than I have generally seen the hearts of frogs make in that time, when their *thorax* was opened without decollation.

5. AT nine minutes past eleven in the forenoon, immediately after decollating another frog, I destroyed its spinal marrow with a red hot wire, which produced terrible convulsions in all the muscles, as in the last experiment. I opened the *thorax* of this frog thirty-five minutes after decollation, and observed its heart beating 30 times in a minute. The contraction of the auricle regularly preceded that of the heart: the auricle was not near so much distended with blood, nor the heart so much swelled as in those frogs which had a solution of *opium* injected into their stomach and guts *. At one o'clock (*viz.* an hour and fifty-one minutes after decollation), the heart of this frog made 20 pulsations in a minute. At half an hour past two, when the room was become warmer by the shining of the sun, it beat 25 times in a minute; and when placed in the sun-beams, it performed 31 contractions in that time. After this, I removed the frog to an east window, where it was exposed to a cool breeze; upon which the motion of its heart became slower, so that in a short time it only made 25 pulses in a minute. I then exposed it anew to the sun-beams, by which its motion was soon quickened, so that it beat 30 times in a minute.

AT twenty-five minutes past five in the evening (*viz.* six hours and sixteen minutes after decollation and the destruction of its spinal marrow) the auricle of this frog's heart, which was still filled with blood, contracted twelve times in a minute; but the heart itself lay without motion, was swelled and very red: however, when pricked with a pin, it performed two or three pulsations, and then remained at rest, till roused by a new *stimulus*. At thirty-five minutes past five, the heart seemed to be quite dead, but the auricle continued its motion; nay, at half an hour past eight, near three hours after the heart had been without motion, the auricle, which was very near as much filled with blood as when I first opened this frog, beat 11 or 12 times in the minute; its pulsations, however, were not now so regular as to time as they had been before.

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* See No. 3. above, and Essay on the vital and other involuntary motions of animals, p. 197.

Is it not probable, that the auricle of this frog's heart beat longer than usual, because it continued, to the last, to be filled with blood; whereas, generally, the auricles of frogs hearts, which are opened after decollation and the destruction of their spinal marrow, expell, after some time, the blood which they contain, and acquire the appearance of a small pellucid bladder filled with air?

6. I laid bare the abdominal muscles and *thorax* of a frog, by dissecting off the skin, and at twenty minutes before nine in the morning, I immersed the whole body of the frog in a turbid solution of *opium* in water, in a small basin, which I covered, to prevent the frog from leaping out of it. Thirty-five minutes after immersion, I took it out of the solution, and opened the *thorax* and *pericardium*. The heart's auricle, which was much distended with blood, beat 15 times in a minute, but the heart itself only 6 times. Forty minutes past nine (*viz.* twenty-five minutes after the frog was taken out of the solution of *opium*) the heart seemed to have recovered more life; for it performed eight pulsations in a minute: the contractions of the auricle now became feebler, and were scarce more numerous than those of the heart, but always preceded them some little time. Six minutes before ten this heart moved only six times in the minute. Twenty-four minutes past ten it made only five pulsations in sixty-five seconds, the first, third, and fifth of which pulsations were after an interval of fifteen seconds, and the second and fourth after a pause of ten seconds. Seventeen minutes before twelve, and two hours and twenty-eight minutes after the frog was taken out of the solution of *opium*, its heart moved only thrice in seventy-five seconds, and performed its *systole* very slowly. Before two o'clock after noon the heart was quite dead; but how long, I cannot say, not having had leisure to observe it from a quarter before twelve to near two.

7. AFTER cutting off a frog's head and destroying its spinal marrow with a red hot wire, I laid bare the abdominal muscles and *thorax*, as in the last experiment, and immersed the whole body of the frog in a turbid solution of *opium*, at half an hour past nine in the morning. Thirty-six minutes after immersion, I took it out of the solution, and opened its *thorax* and *pericardium*. The heart
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and its auricle beat each twenty-six times in a minute, and the pulsations of the auricle preceeded those of the heart regularly. The heart did not appear to be more swelled or redder than in a natural state, and the auricle was not near so full of blood as in Exp. 6. Twelve minutes past ten, *viz.* six minutes after this frog was taken out of the solution of *opium*, its heart beat 27 times in a minute. At eleven o'clock it performed 18 vibrations in that time; and 16 at a quarter before twelve. At two o'clock after noon, the auricle, which, having expelled all its blood, was now only filled with air, continued its motions; but the heart lay at rest. Ten minutes past four, *i. e.* five hours and forty-four minutes after the frog was taken out of the solution, the auricle of its heart beat nine times in sixty-four seconds.

8. I laid bare the abdominal muscles and *thorax* of another frog, and at fourteen minutes past eight in the morning, immersed it as above in a turbid solution of *opium*. Fourteen minutes past nine, I took it out of the solution, and laid open its *thorax* and *pericardium*; after which the heart began to beat at the rate of nine times in a minute: but the auricle, which was greatly distended with blood, made no motion, except in so far as it was agitated a little by the pulsation of the heart: nor were the muscles of the legs or thighs brought into contraction by cutting or tearing their fibres. At half an hour past nine the heart beat only seven times in a minute; and the auricle, which was now pretty empty of blood, and, in place of it, filled with air, had a pulsation as well as the heart. Thirteen minutes before ten, *i. e.* thirty-three minutes after the frog was taken out of the solution, the auricle shewed, at considerable intervals, a very faint pulsation, but the heart lay without any motion.

9. THE same day, after cutting off the head and destroying the spinal marrow of another frog, I laid bare its abdominal muscles and *thorax*; and, at eighteen minutes past ten, immersed it in a solution of *opium*, as above. Eighteen minutes past eleven, I took it out of the solution and opened its *thorax* and *pericardium*, after which the heart began to move at the rate of 8 times in a minute.

Twenty-five minutes past eleven, the heart beat 15 times in a minute; and at twelve o'clock it performed between 13 and 14 vibrations in the same time. At two o'clock, (*viz.* two hours and forty-two minutes after the frog was taken out of the solution), the auricle, which was now filled with air, continued to vibrate weakly about 11 times in the minute; but the heart itself was without motion. At ten minutes before four in the afternoon, the auricle still continued to move, but more feebly than the auricle of N° 5.

10. I laid open the whole *abdomen* of a larger frog than any of the former; and, at twenty-two minutes past ten in the morning, immersed it in a solution of *opium*, as above. Thirty-five minutes after immersion, I took it out of the solution, and opened its *thorax* and *pericardium*. The heart was vastly red and much swelled, and its auricle greatly distended with blood; but both were without any motion: after two minutes, however, the heart began to vibrate at great leisure, scarcely performing nine pulsations in a minute; but the overstretched auricle made not the smallest motion. During every *systole*, the heart was remarkably paler, and in the time of its relaxation became much redder; which appearance I observed likewise in all the frogs hearts in the above experiments, but more remarkably in those frogs who had been exposed to the action of *opium*. Another thing, which I remarked in all these experiments, was, that the heart, during its *systole*, became manifestly shorter, and was lengthened in the time of its relaxation. But to return; at six minutes past twelve, (*i. e.* an hour and nine minutes after the frog was taken out of the solution), its heart made only 6 pulsations in the minute; and at eleven minutes past twelve, observing it without motion, I pricked it with a pin, and breathed upon it, in order to renew its pulsation; but to no purpose.

11. TWENTY eight minutes past seven in the evening, I laid open the whole *abdomen* and *thorax* of a frog, and immediately after immersed it in a solution of *opium* as above. Thirty-eight minutes past seven, when I pricked its legs with the point of a penknife, it made very little motion. Two minutes after this, I turned it to its back,

back, and observed its heart moving only between ten and eleven times in a minute. Having laid the frog again on its belly, that it might be more exposed to the action of the *opium*; at forty-eight minutes past seven, *i. e.* twenty minutes from the first immersion, I turned it again to its back, and observing the heart without motion, I opened the *pericardium*; which producing no effect, I cut the heart out of the body, and laid it on a plate, when it gave two or three pulses, and never after moved, though it was pricked once and again with a pin.

No motion was produced in any of the other muscles of this frog, by irritating them.

12. I cut off a frog's head and destroyed the spinal marrow with a hot wire, then laid open its *thorax* and *abdomen*, and immersed it in a solution of *opium* at nineteen minutes past eleven. Eight minutes before twelve, *i. e.* thirty-three minutes after immersion, I observed its heart beating very slowly: but two minutes before twelve, when I took it out of the solution of *opium*, it had no motion. After this, I opened the *pericardium*, and irritated the heart two or three times with the point of a *scalpel*, which always produced a few pulsations. I then put the frog in the solution for five minutes more, and, upon taking it out, found its heart quite dead.

13. AFTER cutting off a frog's head and destroying its spinal marrow, I laid open its whole *abdomen*, and immersed it in a solution of *opium*, twenty-three minutes before one. After it had lain sixteen minutes, I cut up its *thorax* and *pericardium*; and observing the heart beating very regularly and pretty strongly 21 times in the minute, I immersed it again in the solution, which had now immediate access to the heart. After five minutes, I took it out of the solution; and finding the heart without motion, I pricked it with the point of a knife; upon which it began to beat at the rate of 14 times in the minute, and continued its motions very languidly, and not without some interruption, for about a quarter of an hour.

14. I cut out the heart of a frog, and put it in fountain-water at ten minutes past ten; immediately after immersion, it beat about

28 times in the minute. Eighteen minutes past ten, it made 6 pulsations in thirty seconds. Twenty minutes after ten, I took it out of the water and laid it on a table, and observed, that as often as it was gently touched with any thing, it made one full and strong contraction, and no more: however, in four or five minutes, it began to beat of its own accord, and, at twenty-eight minutes after ten, performed 19 pulsations in a minute. Thirty-five minutes past ten, it beat 12 times in a minute.

15. TWENTY three minutes past twelve, I cut out the heart of another frog, and put it in fountain-water. After twelve minutes immersion, I took it out of the water, when it beat above 20 times in a minute. Having put it in the water for five minutes more, it ceased from motion, and when taken out, did not move except when pricked, and then only performed one pulsation.

16. EIGHT minutes past eleven, I cut out the heart of a third frog, and put it into fountain-water. Eleven minutes after immersion its heart beat 8 times in the minute, and four minutes after this it vibrated 11 times in thirty seconds; but the motion was confined to about one third part of the heart next its *apex*. Twenty minutes after immersion, it continued to move much in the same way; but in two minutes more, observing no motion in it, I took it out of the water, and laid it on a table, where it remained at rest, unless when touched. Soon after this, however, it began to move; and at twenty-five minutes after immersion, it made 9 pulses in sixty-three seconds. Four minutes after this, it moved only thrice in fifty seconds, and then ceased altogether; unless that, when pricked with the point of a knife, it gave one very faint pulsation. At forty-seven minutes past eleven, it was quite dead.

17. I cut out the heart of a fourth frog, and at thirty minutes past ten immersed it in a turbid solution of *opium* in water of the same degree of heat with the fountain-water used in the three last experiments *. After this heart had been immersed ten minutes, I took it out of the solution, and laid it on a table, but it made not the smallest motion; and when pricked with the point of a knife, though

* *Viz.* Nearly sixty degrees of Fahrenheit's thermometer.

though it quickly recovered its shape, yet it was not excited into a proper contraction, as the heart of N° 14. I continued to observe this heart from time to time for more than half an hour, but it never made the least motion.

18. I cut out the heart of a fifth frog, and put it into a solution of *opium* in water five minutes before eight. After seven minutes immersion, I took it out, and laid it on a plate, where it remained at rest. When pricked with a knife, it did not perform a full pulsation like N° 14. but seemed to feel a little, by a very faint kind of motion which was excited in some of its fibres.

19. AT thirteen minutes before twelve, I cut out the heart of a sixth frog, and immersed it in a solution of *opium*. Six minutes after immersion, it had no motion; but when pricked, made one pulsation. After lying five minutes more in the solution, it was quite dead.

20. I cut out the heart of a seventh frog, and at thirty-seven minutes past nine in the morning, immersed it in a solution of *opium*, as above. Forty-two minutes after nine, when I took it out of the solution, it was without motion: but when touched with the point of a knife or probe, it performed one contraction, but with less vigour and more slowly than the heart of N° 14. Forty-seven minutes past nine, it began to beat of its own accord. Two minutes after this, it moved 6 times in the minute, but much more feebly than N° 14. Six minutes before ten, it beat only 4 times in a minute: after this, it began to beat much faster; but its motions soon returned to their former slowness. At ten, after having lain near a minute without motion, it began again, of its own accord, to beat at the rate of 17 times in the minute, and continued for eight or ten minutes after this to beat very feebly, and in an irregular manner as to time.

21. MR ROBERT RAMSAY student of medicine in this place, having dissolved two scruples of *opium* in an ounce of water and a dram of liquid *laudanum*, injected it blood-warm into the *intestinum rectum* of a very small dog near six months old. In less than a minute after the injection was made, the dog could not stand on his
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hinder legs ; and in 3 or 4 minutes he had lost the use of them so much, that when they were strongly pinched, he neither moved them, nor seemed in the least degree sensible of pain. He could, however, still scramble about with his fore-legs ; and when they or his ears were pinched, he howled remarkably, and seemed to feel considerable pain. Ten minutes after the injection, he lay as if he had been quite stupid ; only when a noise was made by beating on the ground, he opened his eyes a little and howled, but presently after fell into a profound sleep. In a few minutes after this, he began to be convulsed ; upon which Mr Ramsay injected a strong solution of sea salt in water into his guts, which purged him severely, and occasioned a *prolapsus ani* ; soon after this, he awaked from his sleep, and gradually recovered the use of his hinder legs ; so that in less than an hour he could run about the room, tho' he often fell down, his legs bending under him. After three or four hours, he seemed to be quite well in every respect ; but altho' the experiment was made at mid-day, he could taste no meat till late at night. When he was in the most stupid state, he could make use of his fore-legs, and complained when his ears were pinched.

22. THE same young gentleman, at my desire, made the following experiment. On the 9th of April 1755, after making an opening into the cavity of the *abdomen* of the dog on whom the last experiment was made, he injected by the wound a dram of *opium* dissolved in two ounces and a half of water ; but before he could stitch up the wound, about an ounce of the solution escaped. The dog lost the power of his hinder limbs almost instantaneously. Two minutes after the injection was made, he began to be convulsed ; and, in two minutes more, after having raised himself upon his fore-legs, he fell down senseless. At this time Mr Ramsay laid bare the *thorax*, by dissecting off the teguments, which did not seem to give the dog any pain, and could plainly feel the motion of his heart thro' the *pleura* : it beat 76 times in a minute, but became gradually slower *. Immediately after counting the pulse, Mr
Ramsay

* The dog's heart in a natural state, and before the injection of the solution of *opium*, beat 150 in the minute.

Ramsay cut the ribs on each side of the *sternum*, which he laid back in the usual way. The heart, which was thus brought in view, appeared quite turgid, and continued in motion about five minutes; during which time it performed only between 60 and 65 weak vibrations, for they were not complete contractions. While the heart was thus moving, warm *saliva* was first applied to it, then cold water, and last of all oil of vitriol; which shrivelled the parts it touched almost in the same manner as a hot iron would have done; but none of them accelerated the heart's vibrations, which became gradually slower, till they ceased altogether.

THE fibres of some of the intercostal muscles on the right side of the *sternum* continued to be agitated with a weak tremulous motion near half an hour after the injection was made into the *abdomen*; but the intercostal muscles attached to the ribs on the sides of the *thorax* were not observed to move, nor did the diaphragm make any motion when its fibres were pricked or cut.

NOTHING remarkable was seen in the *abdomen*; only, altho' it was opened ten minutes after making the injection, the intestines had no motion; whereas, in another young dog, which had got no *opium*, Mr Ramsay observed the peristaltic motion continue half an hour after laying open the *thorax*.

THE dog lost little or no blood in making the wound into his *abdomen*, nor were any of his bowels hurt by it.

23. A small dog into whose stomach the late celebrated Dr Mead had forced, at four different times, a solution of two drams of *opium* in water, lived above an hour and three quarters after getting the first dose. *Vid.* treatise on poisons, Essay IV.

24. IT may not be improper to add here an experiment related by Dr Alston in his learned dissertation on *opium* *. Into the crural vein of an old dog forty-two pounds weight, he caused be injected at three different times, half an ounce of *opium* dissolved in four ounces of water, filtrated, and of the same warmth with the blood of the animal. The first time, about fifteen drams were thrown in, and very slowly. It had no observable effect. About an hour after,

* Edinburgh Med. essays, vol. 5, p. 1. art 12.

after, eight drams more were injected slowly, and immediately the dog was seized with strong convulsions: the pulse was frequent and small, and after some time he foamed at the mouth. But there appearing no signs of immediate death, after waiting an hour more, the last nine drams were thrown in quickly; upon which the pulse became full and slow, and in a minute or so, the dog expired.

FROM the preceeding experiments, we may, I think, fairly draw the following conclusions.

(a) OPIUM applied to the stomach, guts, cavity of the *abdomen*, and *thorax*, and abdominal muscles, soon lessens, and after some time intirely destroys all feeling and power of motion, not only in the parts to which it is applied, but thro' the whole body. N° 1. 2. 3. 8. 11. & 22.

(b) OPIUM produces these effects much more quickly in animals which are soon killed by want of food and air, than in those which can live long without them, and the parts of whose bodies preserve a power of motion and appearances of life for a considerable time after they are separated from each other. N° 1. 3. &c. compared with N° 21. 22. & 23.

(c) SINCE a solution of *opium* injected into the stomach and guts destroys the sensibility and moving power of frogs, fully as soon when they are deprived of their heart, as when this organ remains untouched; it follows, that *opium* applied to these parts does not produce its effects by entering the blood, and being, by its means, conveyed to the brain, as some have imagined, but by its immediate action on the organs and parts which it touches. N° 1. compared with N° 2. See also Edinburgh Medical essays, edit. 3. vol. 5. part 1. page 140.

(d) SINCE, after decollation and the destruction of the spinal marrow, *opium* operates much more slowly in destroying the heart's motion in frogs, than it does when the animals are intire (No. 6. compared with No 7.); it follows, that it must produce its effects chiefly, if not wholly, by its action on the brain, spinal marrow, and nervous system. The heart of the frog N°. 7. whose brain and
spinal

spinal marrow had been destroyed, beat 27 times in a minute, after the animal had lain thirty-six minutes in a solution of *opium*; which was only three pulsations less than the heart of the frog N^o. 5. performed thirty-five minutes after the destruction of its brain and spinal marrow, although it was not exposed to the action of *opium*.

(c) WHEN *opium* injected into the veins, and thus mixed with the blood lessens or destroys the sensibility and moving power of animals, much in the same way as when it is applied to their stomach, guts, or cavity of the *abdomen* (N^o 24.); is it not probable, that it produces these effects by its action on the extremities of the nerves which terminate upon the internal surface of the heart and whole vascular system; and perhaps, also, by affecting immediately the *medulla cerebri* itself? And when a solution of *opium* applied to the bare abdominal muscles of a frog deprived of its brain and spinal marrow, does, after a long time, considerably impair the heart's motion; is it not reasonable to think, that this is owing to the finer parts of the *opium* being absorbed by the bibulous veins and carried to the heart, and thus brought into contact with the nerves of this organ? N^o. 7. compared with N^o. 9.

(f) SINCE *opium*, without entering the blood, or being carried to the several parts of the body, destroys the power of feeling in animals *merely* by acting on the nerves to which it is applied (c), (d), it follows, that the nerves are the instruments of sensation, or, at least, necessary to it. Nor is it sufficient to destroy this conclusion, that there have been instances of animals endowed with feeling whose brains were so greatly diseased, as to seem incapable of performing their functions. It is far from being safe to build theories in physic upon a few monstrous appearances in nature.

(g) IT appears from N^o 4. and 5. compared with N^o 3. 6. 8. 10. and 11. that decollation and the destruction of the spinal marrow does not weaken or destroy the heart's motion in frogs, near so soon as *opium* injected into their stomach and guts, or applied to the muscles and bowels of the lower belly and *thorax*.

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(h) ALTHOUGH

(b) ALTHOUGH a solution of *opium* applied to the opened *thorax* and *abdomen* of a frog, after decollation and the destruction of its spinal marrow, soon weakens or destroys the motion of the heart; yet it does not produce these effects so speedily as when the brain and spinal marrow are intire, N° 11. and 12. In the former case, the *opium* can only affect the heart by its topical influence; in the latter, it not only acts this way, but also exerts its powers upon the brain, spinal marrow, and whole nervous system; and therefore must produce more sudden effects.

(i) IT appears, beyond doubt, from the preceeding experiments, that the heart is not exempted from the power of *opium*, as the learned Dr Haller has affirmed *, but has its motion destroyed by it, as well as the other muscles, only not so soon. See No. 4. and 5. compared with N° 3. 6. 8. and 10.; and N° 14. 15. and 16. compared with N° 17. 18. 19. and 20.

'TIS true, that the fibres of the intercostals on the right side of the *sternum* of the dog N° 22. continued to be agitated with a tremulous motion considerably longer than the heart, and when the intercostal muscles attached to the ribs were quite dead. But did not this happen because, after separating the *sternum* from the ribs, and thus cutting off all communication between it and the spinal marrow, the muscles attached to it could be no more affected by the *opium*, which had been injected into the cavity of the *abdomen*; while the heart and other muscles, whose communication by means of the nerves with the brain and spinal marrow was intire, continued to be exposed to its action?

(k) As Dr Langrish has observed, that the distilled water of laurel leaves injected into the cavity of the *abdomen*, kills dogs sooner than when it is taken into the stomach †; so N° 21. and 23. compared with N° 22. shew that *opium* injected into the stomach and great guts of dogs, does not produce either such speedy or powerful effects, as when thrown into the cavity of the *abdomen*. And N° 6. compared with N° 10. shews, that a solution of *opium* applied to the abdominal

* Act. Gotting. vol. 2. p. 147. & 154.

† Physical experiments on brutes, p. 64.

dominal muscles, does not kill frogs so soon as when all the *viscera* of the lower belly are exposed to its action.

(l) ALTHOUGH it seems probable, from N° 22. compared with N° 24. that a solution of *opium* injected into the veins of dogs does not kill them so soon as when thrown into the cavity of the *abdomen*; yet this cannot be certainly concluded, since the dog of N° 24. was much older and above ten times heavier than the other.

(m) IT appears, that a solution of *opium* injected into the great guts of a dog, affects the inferior part of the spinal marrow much more remarkably than its superior part, or the brain; since the dogs of N° 21. and 22. not only lost the power of motion sooner in their hinder legs than in their fore ones, but also were insensible of any pain in them, and yet howled strongly when their ears were pinched.

(n) A solution of *opium* injected into the cavity of the *abdomen* or great guts of dogs, does not destroy the feeling and power of motion of their hinder limbs, by sending any *effluvia* to their muscles; otherways it could not produce these effects so instantaneously, (N° 21. and 22.) Besides, since *opium* thrown into the stomach and guts of a frog after being deprived of its heart, destroys the sensibility and moving power of its muscles equally soon as if the animal had been intire (N° 2.); it is plain, that these effects cannot be owing to the finer parts of the *opium* being received into the blood, and by its means carried to the several muscles and organs.

(o) NOR does a solution of *opium* injected into the great guts or cavity of the *abdomen* in dogs produce its effects by transmitting through the nerves any subtile *effluvia* to the spinal marrow; otherways its operation could not have been so instantaneous (N° 21. and 22.); nor could the spinal marrow and its nerves have recovered their functions so soon after the *opium* was evacuated by a purgative clyster, N° 21.

(p) IT remains, therefore, that *opium*, by affecting the extremities of the nerves of the parts to which it is applied, does, by means of their connection and sympathy with the brain and spinal marrow, destroy or prevent, through the whole nervous system, the operation

tion of that power upon which depends sensation and motion in the bodies of animals.

(*q*) SINCE *opium* applied to the abdominal muscles of a frog deprived of its brain and spinal marrow, does not destroy the motion of the heart so soon as when it is applied to the abdominal muscles of a frog whose brain and spinal marrow are intire, (N^o 6. and 7.), it follows, that the brain and spinal marrow, and consequently the nerves derived from them, have a greater influence than any other part of the animal system upon the motion of the heart.

(*r*) OPIUM does not only destroy the moving power of the muscles of animals by intercepting the influence of the brain and spinal marrow, but also by unfitting the muscular fibres themselves, or the nervous power lodged in them, for performing its office: otherways a solution of *opium*, when applied to the abdominal muscles or *viscera* of a frog, would not put a stop to the heart's motion sooner, or indeed so soon, as decollation and the destruction of its spinal marrow, (N^o 4. and 5. compared with N^o 8. and 10.). *Opium* therefore does not produce its effects solely by putting a stop to the function of the brain and spinal marrow, but its influence reaches to the fibres of the muscles themselves, or to the extremities of the nervous filaments which terminate in them.

WHEN I say the influence of *opium* reaches to the nervous filaments which terminate in the muscular fibres, it is not meant, that any *effluvia* or subtile parts of the *opium* are transmitted to them, (See *n* and *o* above), but that it destroys their powers by means of that sympathy which they have, through the brain or spinal marrow, with the nerves to which the *opium* is immediately applied.

(*s*) FROM the above experiments we may infer, that not only the power of voluntary motion in the muscles, but also their irritability or power of motion, when stimulated, proceeds from the nerves, or is at least immediately dependent on their influence; since *opium*, which produces its effects solely by affecting the nervous system, (*m*, *n*, and *o*), destroys those powers so suddenly. I know it has been lately argued by a celebrated author, that the irritability
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of the muscles must be independent of the nerves, because the muscles of animals preserve a power of moving when irritated, for some time after the communication between them and the brain, by means of the nerves, is cut off *. But since a solution of *opium* applied to the abdominal muscles of frogs, merely by its action on the nerves, puts a stop to the irritability or moving power of the heart, much sooner than the destruction of the brain and spinal marrow (*g*); is it not reasonable to conclude, that the tremulous motions of irritated muscles, after their nerves are tied, proceed from the integrity of the nervous filaments below the ligature, and the nervous power still remaining in them or in the muscular fibres themselves?

THE tying or cutting of a nerve, only prevents the derivation of any new influence from the brain to the parts to which it belongs; but does not immediately destroy the power or influence remaining in the nerve itself. *Opium* applied in sufficient quantity to the sensible parts of animals, not only quickly puts a stop to the function of the brain and spinal marrow, and thus produces in the muscles all the effects of a ligature on their nerves, but also destroys the power of every nervous filament in the body (*r*); and therefore puts a stop to the motion of the heart in frogs sooner than the destruction of the brain and spinal marrow.

(*t*) THE almost instantaneous palsy brought on the hinder legs of a dog, by injecting a solution of *opium* into the cavity of its *abdomen*, (N^o 22.), and the effects of the same solution injected into the stomach and guts of a frog deprived of its heart, (N^o 2.), where no part of the *opium* could be conveyed to the muscles, nor be conceived to alter the nature of their *gluten*, shew, that the irritability of the muscles has not its seat in this *glue*, as some have lately imagined †. But if the motions of irritated muscles be owing to a disagreeable sensation excited in them or their nerves, as we have elsewhere endeavoured to shew ‡, it is easy to see that *opium* must, by

* Act. Gotting. vol. 2. p. 134. &c.

† Ibid. p. 152.

‡ Essay on the vital and other involuntary motions of animals, sect. ix.; and Physiological Essays, p. 302.

by destroying the sensibility of the muscles, of consequence also destroy their irritability.

(u) IN animals which have got a large dose of *opium*, the veins, especially those of the membranes of the brain, are observed to be much swelled; whence it has been thought, that *opium* produces its effects in the bodies of animals, partly, at least, by rarefying the blood and compressing the brain: but this distension of the veins seems to be no more than a consequence of the very slow motion of the blood through the heart, on account of the insensibility with which this organ is affected *.

(v) SINCE *opium* soon puts a stop to the vital motions of animals, which yet continue in time of sleep with little or no diminution of their vigour; since it often eases pain without bringing on sleep; and since, by its topical action on the heart, it destroys the motion of this organ after all communication between it and the origin of the nerves is cut off †; it follows, that the effects of *opium* are not owing, as some have thought, to its producing sleep: on the contrary, the sleep which it occasions seems to be only a consequence of its impairing the sensibility of the whole nervous system.

THE other effects of *opium* may be also deduced from the same cause, particularly its restraining all evacuations that are owing to an unusual irritation of the parts of the body, and at the same time promoting those natural secretions which have been diminished or stopt by spasmodic strictures of the vessels, from some uncommon *stimulus* affecting them.

(w) LASTLY, does not *opium* kill animals by rendering their several organs wholly insensible of the *stimuli* which are destined by nature to excite them into action; whence not only a stop is put to the peristaltic motion of the guts, and to the propulsion of the chyle ‡, but the fluids also being to stagnate first in the smaller and afterwards

* In frogs, into whose stomach and guts I had injected a solution of *opium*, I not only found the heart's auricle, but also the great veins leading to it, much distended with blood. Vid. Essay on vital motions, &c. p. 197.

† Vid. No. 12. 13. 17. 18. 19. & 20. above.

‡ In a small dog, which Dr Kaau Boerhaave opened, after having given him three grains of *opium*, he observed scarce any peristaltic motion in the guts: the stomach was much distended;

afterwards in the larger vessels *; while the heart becoming gradually less sensible of the *stimulus* of the blood with which it is distended, contracts more feebly and at greater intervals, till at last it ceases from motion altogether?

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tended; the *pylorus* was shut, and the bread and milk, which the dog had taken with the *opium* about ten hours before, was indigested. There was nothing like chyle in the *duodenum*, nor any lacteal vessels to be seen in the mesentery. The bladder of urine and great guts were much filled, nor had the animal evacuated either urine or *feces* from the time he swallowed the *opium*; *Impetum faciens Hippocrati dictum*, p. 402. & 403. The learned Dr Haller has also observed, that *opium* puts a stop to the peristaltic motion of the guts in frogs and other animals; Aët. Gotting. vol. 2. p. 154.

• This my worthy Colleague Dr Alston observed with a microscope in frogs into whose stomach he had conveyed a few drops of a solution of *opium* in water. *Vid.* Medical Essays, vol. 5. part 1. art. 12. And indeed the great distension of the heart and its auricle in frogs killed with *opium* (No. 5. compared with No. 3. 6. & 10. above) indicates a more than ordinary resistance to the blood's motion in the arteries, as well as a less degree of irritability in the heart. Further, is not the slow full pulse, and dry parched mouth, in those who have got an over-dose of *opium*, owing partly to the slower motion of the fluids in the small arteries and secretory vessels of the glands? Though it must be confessed, that the dryness of the mouth may be in some measure owing to the perspiration being greatly increased by the *opium*.

AN
ESSAY

ON THE
VIRTUES of LIME-WATER and SOAP

IN THE
CURE of the STONE.

Non fingendum aut excogitandum, sed inveniendum. BACON.

AN

ESSAYS

ON THE

VIRTUES OF LIME-WATER AND SOAP

IN THE

CURE OF THE STONE

Non facendum est excrucians, sed curandus. Hicet.

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T O
H I S G R A C E
ARCHIBALD DUKE OF ARGYLL,
&c. &c. &c.

The following E S S A Y

is inscribed,

With the greatest Respect,

B y

H I S G R A C E's

Most devoted, and

most obedient

humble Servant,

ROBERT WHYTT.

TO

HIS GRACE

ARCHBISHOP DUKES OF ARGENT

Sec. Sec. Sec.

The following ESSAY

is intended

With the greatest respect

BY

HIS GRACE

Most Reverend, and

most obedient

servant

ROBERT WHITT

THE

ADVERTISEMENT.

THE following essay was first published in the year 1743, in the Edinburgh Medical Essays, vol. 5. part 2. and met with a more favourable reception from the public, than the author, considering its imperfections, had reason to expect. It is now printed by itself, at the desire of several of his friends, who assured him, that, in this way, it might be of more general use, since many persons, afflicted with the stone, would chuse to read it, who did not incline to purchase so large a book as the Medical Essays: And the author, to render it less unworthy of the favour of the public, has not only corrected, but greatly enlarged it.

THE principal additions are to be found in sect. 2. 3. 9. 10. 11. 12. and 13. and relate chiefly to the nature of quick-lime and its water; the strength and specific gravities of different lime-waters; the particular action of lime-water in dissolving the stone; and to the cure of the stone, not only by swallowing soap and lime-water, but by injecting the latter into the bladder.

THE appendix contains the case of the Right Honourable Horace Walpole, Esq; written by himself, and given me, at his desire, by the Honourable Mr Baron Edlin, of his Majesty's Court of Exchequer in Scotland, with liberty to make it public.

I might have added many other histories of the good effects of lime-water in the stone, had I not been unwilling to swell this essay to too great a bulk: And, indeed, I thought this the less needful, as the usefulness of this remedy is, within these few years, become
pretty

pretty well known in South as well as North Britain. I have chosen, however, to insert Mr Walpole's case preferably to any other, not only because the good effects of the medicines were here very remarkable, but as it is written by himself, and as the histories of those in conspicuous stations of life are wont to make the strongest impressions upon the generality of mankind.

If this essay, with these improvements, and as it is now published, shall become of more general use, and serve to relieve any of his brethren of mankind from the racking pains of the stone or gravel, the author will think his time and labour abundantly rewarded.

August 1. 1752.

A D V E R T I S E M E N T

To the second EDITION.

IN this edition the author has corrected the errors of the former, and made several additions. He has added to the appendix the cases of the Rev. Dr Newcome and Mr Young Green, which he thought well worth publishing; the latter on account of the uncommon texture of the stone; and the former as it shews that lime-water alone, drunk to the quantity of two English quarts a-day, communicates to the urine a power of dissolving the stone out of the bladder, and consequently must act more powerfully upon one lodged there, where the degree of heat must increase its dissolving power; at the same time that this cannot be lessened by the access of the external air, which, as it soon weakens the lithontriptic power of lime-water, must do the same by the urine impregnated with its virtues.

NOVEMBER 6.

1754.

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A D V E R T I S E M E N T

To the third EDITION.

IN the third edition, several things less material, and some conjectures concerning the nature of quick-lime, which appeared not to be well founded, are omitted; and the appendix is enlarged with a continuation of Lord Walpole's case, and observations on it; with an addition to the case of the Bishop of Llandaff; and with the cases of William Hay, Esq; Mr L. Trevigar, and T. W.

EDINBURGH, June 9.

1761.

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ESSAY

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AN
ESSAY
ON THE

VIRTUES of LIME-WATER

IN THE

CURE of the STONE.

IN the year 1739, the parliament of Great Britain, in consequence of a petition presented to the House of Commons by Mrs Joanna Stephens, generously ordered her a reward of L. 5000 Sterling, for publishing her medicines against the stone, in case, upon due trial, their utility, efficacy, and dissolving power, should be attested by the trustees named for that purpose. But although the virtue of these medicines is such as may justify the certificate given of them; yet they are so bulky and nauseous, that some people cannot take them, and many use them with the utmost reluctance; nay, (as the learned Dr Jurin has well observed), so great is the resolution required to go through a course of these medicines, and so many are the difficulties attending it, that there have been not a few instances of such as, after taking them for many months, without any benefit, have submitted to be cut, rather than go on any longer with a medicine so extremely nauseous, and which had greatly increased their pains, without bringing any thing away*.

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* Dr Jurin's case, p. 4. and 5.

DR HARTLEY, * leaving out the superfluous useless part of Mrs Stephens's compositions, has reduced them to two ounces and a half of soap, and seven scruples and a half of egg-shell powder, as the mean dose to be taken every day. But the powder is so nauseous, and this quantity of soap is so great, that there are but few patients who will continue to take the medicines thus reformed by him for any considerable time †.

AFTER reading the ingenious experiments published (1741) by the Reverend and learned Dr Hales, upon Mrs Stephens's medicines, I was led to think that lime-water had as fair a chance as any thing to dissolve the stone. For since it there appears, that the soap owes its virtues neither to the potash nor oil, but wholly to the quicklime that enters into its composition; and as Mrs Stephens's powder which she used long before she gave the soap in any quantity ‡, and which she still lays great stress upon, is flaked lime; it is very reasonable to expect great benefit from lime-water; which has this further advantage, that by its means the virtues of a greater quantity of lime may be safely conveyed into the blood; for in soap the lime bears so small a proportion to the other ingredients, that but a very inconsiderable quantity of it can in this form be mixed with our fluids, and of the powder (greatly weakened by being exposed sixty days to the open air) only a few scruples are exhibited a day. If this powder is swallowed without enough of liquids along with it, it may be of bad consequence, and has doubtless been the occasion of great heat and uneasiness in some people's stomachs; and if it is sufficiently diluted, especially with white-wine,

* See his supplement to the view of the present evidence.

† Dr Hartley has lately for such as cannot take the soap and powder in a liquid, published the following method of giving them in a solid form, viz. To take of Alicant soap, eight ounces; powdered quick lime a little flaked, one ounce; salt of tartar, or purified potash, a dram; and shaving the soap, and mixing it with the lime and salt, to beat all into a soft mass, with the help of as much water as is necessary: of this mass, from three to four ounces are to be taken every day, made into rolls a little taper at each end, which are to be laid length-ways on the tongue, and swallowed with a mouthful of water. In this way the quantity of soap to be taken daily, is nearly from two ounces and five drams, to three ounces and a half; of lime powder, from three drams to near half an ounce; and of salt of Tartar, or purified potash, from a scruple to near half a dram.

‡ Hartley's supplement to the present view, p. 1.

wine, cyder, or other acefcent liquors, as Mrs Stephens directs, it can have little other effect than weak lime-water. When to all this it is added, that lime-water is found by experience to diffolve a ftone out of the bladder, is it not reasonable to expect, that when taken in large quantities, and but little weakened by the drinking of other liquors, the urine likeways may be fo far impregnated with its virtues, as to acquire a power of diffolving the ftone?

BUT as reasoning *a priori*, unsupported by experience, is not fufficient to afcertain the virtues of any medicine, I embraced the firft opportunity that offered to make a trial of lime-water; the effects of which will beft appear from the following hiftory.

MR DAVID MILLAR, mafter of the grammar-fchool of Kirkaldy, about fixty years of age, had, from the year 1704, been often diftreffed by ftones paffing from the kidneys to the bladder. Sometimes he had fevere fits of pain once or twice in a year, and fometimes but once in two or three years, and thefe of two, three, or four, and even of eight or fourteen days continuance; but always in a few days after thefe fits he voided one or more ftones till June 1740; when, after a fit of fevere pain and ficknefs for two days, the ftone arrived at his bladder; but though he ufed his ordinary means of riding, walking quick, jumping, and drinking plenty of proper liquors to make it pafs, yet all his endeavours were in vain.

FOR half a year after this, he was troubled with frequent obftructions in making urine, although without any great ftimulating pain, except in voiding the two or three laft drops. Afterwards he thought he found the ftone increafe, and become heavier in his bladder; and from March 1741, upon riding or walking a mile or two, his urine was always mixed with blood: befides, from the beginning of January he had loft all power of retaining his urine, being obliged to pafs it every eight or ten minutes, and not without great ftimulating pains; yet fometimes with intervals of eafe for a day or two, after sweating and keeping warm.

AT firft he drank milk and water; but in May 1741 he began to take foap, firft to the quantity of half an ounce every day, which,

in the end of July, he increased to an ounce, and in the beginning of September to near an ounce and an half; but all this without any sensible relief; his pain, bloody urine, and inability to retain his urine, still continuing as before.

IN the end of September, I advised him to drink with the soap large quantities of lime-water, beginning with one, and gradually increasing the quantity to three English pints a-day, and at the same time to drink no more of any other liquors than was necessary to quench his thirst.

WITHIN four or five days after he began to drink the lime-water, he recovered in a great measure the power of retaining his urine; and from that time had less pain and less bloody urine, upon using exercise, than formerly; so that on November 13. although he walked upwards of six miles at a pretty quick pace, yet he retained his water for nine or ten hours together; and as he voided it with little or no pain, so he found no blood mixed with it.

NOVEMBER 15. at night, when going to bed, and trying to make water, he found a stone entering the beginning of the *urethra*, and obstructing it; which it continued to do all night. He slept little, and often attempted to pass urine; but could not, unless a very little, and that drop after drop. Next morning, when he was putting on his cloaths, finding an inclination to make water, and endeavouring it with all the force he could, he voided a smooth stone, about the bulk of a common bean, of a whitish sandy colour; whereas all those he had passed formerly, were of a brown colour, and rough. It appeared plainly to be a part of a larger stone.

UPON the 17th of November he walked upwards of two miles without any pain or bloody urine.

NOVEMBER 18. after making urine he felt something at the neck of his bladder, occasioning a slight obtuse pain, which he took to be another stone.

FROM this till the beginning of December he was very easy, not having been obliged, above three or four times a-day, to make water; which was never mixed with blood, nor attended with those
stimu-

stimulating pains he formerly had. Only twice or thrice he found his urine suddenly stopt, when he was voiding it; and once he thought a stone was entering the passage, which a little after fell back into the bladder. When he stumbled, or stept down a stair, he still felt something heavy that pushed or touched him in the under part of the bladder. His urine during all this time had a great deal of white sediment, and some brownish flakes among it; but he was so much abroad about his business, that he could not make any regular observations upon it.

UPON Thursday night, the 3d of December, the stone which he supposed to have been still in his bladder entered the beginning of the *urethra*; where it stuck till Monday morning following: During which time his urine was very much obstructed, coming away in drops, or in a very small stream, with a good deal of uneasiness and pain. From this to the end of December he was often in the same condition; the stone sticking in the passage sometimes half a day, sometimes a whole day and a night, and then falling back into the bladder; but all these times he never had any of those piercing stimulating pains which he was wont to feel, before he used the lime-water, in passing his urine, and especially after the last drops; and now also he was able to retain it half a day, and then void it without pain. Immediately after emptying his bladder, he always sensibly perceived the weight and pressure of the stone, if he but walked a little; but when there was any considerable quantity of urine in it, this became less perceptible. He concludes a letter to me at this time with these words: "As I have hitherto
 "enjoyed a good degree of health, so now I am easy beyond ex-
 "pectation, which makes me think the stone in a dissolving state,
 "and that its surface is very much smoothed. I continue using the
 "soap and lime-water daily; which last I frequently take to my
 "meat, instead of other drink, and I think my urine tastes a little
 "of it."

ON Monday, January 4. at night he found a stone had got into the beginning of the *urethra*, which in a good measure hindered him from voiding any urine. However, next morning, after a
 good!

good sleep, it came away. It was larger than the one he passed before, and appeared evidently to be a piece of the same stone.

For some days after passing this stone, he found the *urethra* very tender, and a little pained; which occasioned his making urine more frequently than usual. But this soon went off; and he has ever since, to use his own words, been perfectly free of all pains and symptoms of the gravel, and as easy in that respect as ever he was in his life: and upon the whole, concludes, that he received more benefit from the lime-water, than any thing he ever used, and to it chiefly ascribes what has happened above.

As authors have sometimes been accused of framing histories to support a certain theory, or raise the value of some favourite medicine, I thought it might be proper to add the gentleman's own attestation of the truth of what has been above related.

Kirkaldy, June 1. 1742.

“HAVING read the history of my case drawn up by Dr Whytt, I do here, for the satisfaction of the public, declare, that it is in every particular agreeable to truth: and that at present I am as perfectly free of all symptoms of the gravel as ever I was in my life.

DA. MILLAR.”

UPON this history it is natural to observe,

I. THAT from the figure and shape of the stone first passed, it evidently appeared to be a fragment of a larger one that had lain in the bladder for about seventeen months, but was then broken, and in some degree dissolved; and as of this no other cause can be assigned, but the soap and lime-water, it is but reasonable to ascribe it to their efficacy. That part of the stone where it has been broke off, has its edges so sharp, while every where else it is smooth and rounded, and a red *nucleus* so plainly appears in the middle of it, that no reasonable doubt can be had of its being part of a larger stone. But this is put beyond all question by the stone last passed, which tallies pretty exactly with the former, is evidently of the same

same texture, and the two together, excepting a small deficiency at one end, seem to make up a compleat stone. Whether what is wanting has passed in fragments unobserved, or has come away quite dissolved in flakes and white sediment, is not so certain. Farther, as Mr Millar has had no stones passing from his kidneys to his bladder since June 1740; if we deny these stones to have been one, we must suppose them lodged eighteen months in the bladder, without either acquiring a greater bulk, or ever endeavouring to pass; none of which suppositions are at all likely.

2. THE surfaces of the stones shew them to have been in a dissolving state. There are fibres like roots which run along them, in some places plainly broken off, and the hollow furrows they had made still remaining. And as the lime-water and soap had longer time to act upon the second stone, so we find still more evident signs of dissolution about it. It has in most places a very rotten appearance, and in some is eaten so deep, that one can see several of its concentric layers. Further, as Dr Hales has observed, (which will more fully appear in the following experiments), that the *calculus* generally turns white in dissolving, the whitish colour of these stones makes it probable they were in a dissolving state.

3. IT does not appear that the soap alone had any great influence in this case: for although from the end of July, till about the 8th of September, he took an ounce every day, and after that near an ounce and an half; yet he found no sensible relief; which probably might be owing to his not taking it in so large quantities as some others have done *.

4. IT would seem that lime-water has a very uncommon efficacy in easing the symptoms, and probably dissolving the stone in the bladder: For in five days after Mr Millar began to drink it, he was able to retain his urine better than he had done for eight or nine months before, and found the stimulating pains in making it less, and the quantity of blood in it sensibly to diminish; so that upon the 13th of November, although he walked six miles pretty quickly,

* The mean quantity ordered by Mrs Stephens is two ounces and a half.

ly, he had none of it, nor has had ever since; and on the 16th of the same month (having used it little more than six weeks) he passed the first stone. Nor is it at all unreasonable to suppose that lime-water may have a greater effect in dissolving the stone than soap, since we find it possesses a greater dissolving virtue out of the bladder. See experiment below, Sect. 3. compared with N^o 70.

THAT the lime-water, from its healing and strengthening quality, should have a more sudden effect in curing the incontinency of urine, than the pain and blood that, upon motion, generally accompanied it, is easily accounted for; since, as the two last proceeded chiefly from the rough sides of the stone grating upon, and tearing the blood-vessels on the internal coat of the bladder, this must in part have continued to happen till the points were worn off its surface; and accordingly we find the first stone was pretty smooth when he voided it. But, besides this, after the stone once begins to dissolve, its surface is either covered with a whitish *mucus*, or soft rotten scales or layers, which are thrown off one after another. *Vid.* Exper. below.

5. IT may not be amiss to take notice, that although Mrs Stephens's medicines almost always occasion great pain and heat of urine for some weeks, or even months, after first taking them*; yet the soap, in the way Mr Millar took it, gave him no such uneasiness; and the lime-water had so very contrary an effect, that in a few days it relieved some of his complaints, and abated others. As the pain and heat of urine in taking these medicines seem chiefly owing to the alkaline salt, which bears so great a proportion in the composition of the soap, is it not probable, that it was from his beginning to take the soap in very small quantities, and gradually increasing the dose, as well as never taking so much of it as is ordered by Mrs Stephens, that Mr Millar escaped this complaint?

FINDING his stomach unable to bear the soap in decoction, he took it every morning in substance, only sliced down; and found it

* See Dr Hartley's view of the present evidence, &c.; and Dr Kirkpatrick's case, written by himself.

it agree very well with him, excepting that it sometimes gave him a little of the heartburn. And the lime-water and soap were so far from having any bad effect upon his health, that he rather found himself easier and lighter, and considerably freer of some kind of lowness of spirits he had before laboured under.

As Mr Millar continued from the beginning of the year 1742, when this paper was first written, to June 1751, when he died, perfectly free of all symptoms of the stone, without taking medicines of any kind; so there can be no doubt, but that the stone, which gave him so much uneasiness, was wholly brought away by the use of the lime-water and soap; for if any part of it had remained, it must, in so long a time, have acquired bulk sufficient to have produced the like uneasy symptoms with which he had been formerly afflicted. And here it is worthy of observation, that Mr Millar, by the use of these medicines, was not only cured of the stone in the bladder, but rendered, during the remainder of his life, almost quite free of nephritic complaints, to which he had been much subject for many years before.

By this success of the lime-water, I was induced to make the following experiments, with a view to a further discovery of its nature and virtues.

S E C T. I.

Experiments with quick-lime.

MALT-SPIRITS poured upon a piece of fresh calcined lime-stone, are plentifully absorbed by it, without any sensible ebullition, (some air-bubbles excepted, that arise from its surface); nor is it flaked but after lying a great many hours in them.

I immersed a piece of quick-lime in rectified spirit of wine, in a close stopped bottle, which, after eight days, scarcely shewed any appearance of being flaked.

2. VINEGAR is somewhat more plentifully absorbed by quick-lime

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than

than spirits, with a good many air-bubbles, and hissing noise at first, which however soon ceases; and if the lime is fresh from the fire, it will scarcely be flaked, unless allowed to continue many hours in it.

WATER, whether cold or hot, poured upon quick-lime that has lain some time among spirits or vinegar, produces no ebullition, except a very few air-bubbles at first; and the stone that has been in the vinegar is longest in being flaked.

3. OIL is plentifully imbibed by quick-lime, without any ebullition or heat. If there is any rent in the stone, a few air-bubbles will sometimes rise out of it. When put in boiling water after this, a good many bubbles of oil rise from the surface of the stone; and, after several hours standing, it begins to melt down into a soft, fat, argillaceous substance *.

4. UPON putting a piece of quick-lime in claret wine, a considerable ebullition immediately happens; but in twenty-four hours the stone is scarce dissolved.

A piece of quick-lime being put into brisk strong ale, a good many air-bubbles immediately arose with some noise; but this was quickly over. After twenty-four hours one third of the stone was not flaked.

SMALL beer has much the same effect; only the ebullition is greater, and lasts longer.

WATER, whether cold or hot, poured upon a piece of quick-lime that has lain some time in ale or beer, produces no ebullition, nor easily dissolves it.

5. THE heat or cold produced by the mixture of the above liquors with powdered quick-lime, I found to be as follows.

WHEN Quick-lime was mixed with lamp-spirits, the thermometer fell, in two or three minutes, from 54 to 53 degrees.

WITH

* Oleum solum calci miscetur, quando utrumque aquas odit. Plin. Histor. natur. lib. 24. cap. 1.

Calx aqua accenditur, eademque oleo restinguitur. Id. lib. 33. cap. 5.

Si oleum rosarum vel liliorum alborum vel lini probe agitetur in mortario cum aqua calcis, ambo coalescunt in modum butyri. Etmuller, oper. vol. 2. p. 799.

WITH vinegar it rose, in five minutes, from 52 to 68 degrees; after which it began to fall.

WITH claret it rose, in six minutes, from 51 to 56 degrees.

WITH strong ale it rose, in ten minutes, from 48 to 57 degrees.

WITH cold water it rose, in twenty-two minutes, from 48 to 112 degrees, and then began to fall *.

6. TEN times its weight of boiling water being poured upon fresh burnt lime-stone, there immediately arises a great ebullition, which lasts a considerable time; when this is over, the lime falls to the bottom, and the clear water above, being filtrated through brown paper, is that which I made use of in the following experiments. The proportion of 8 to 1, ordered in the Edinburgh dispensatory, seems rather too small, as it affords but very little water, if the lime-stone be well burnt, and fresh from the fire: Nor have I observed any remarkable difference in the strength of the lime-water made with these different proportions.

COLD water being added to quick-lime, soon produces a considerable heat and ebullition, and this water has the same virtues as the above.

7. IF quick-lime be mixed with fresh urine, it instantly sends forth a pungent vapour, which strikes the nostrils in much the same manner as volatile ammoniac salt.

S E C T. II.

Experiments with lime-water upon urine.

AS the calculous concretions of the kidneys and bladder are generated by the urine, and owe their growth entirely to a constant apposition of particles derived from this fluid, I thought it might be worth while, before making any experiments on the *calculus*, to try the effects of lime-water upon urine and its sediment.

8. IF two ounces of lime-water be added to an equal quantity of fresh-made urine, the mixture instantly becomes whitish and turbid;

X x 2

* The quantity of quick-lime used in this experiment was very small, otherways the thermometer would have risen much higher.

bid; and, in a little time, a light white sediment falls to the bottom, leaving the liquor above perfectly pellucid, of a fine light limon-colour *, without any scum or crust on the sides of the glass.

9. I let some fresh urine stand by itself in a glass about forty-eight hours; in which time it had deposited a redish brown sediment upon the bottom of the glass, with a crust of the same nature upon its sides. I then decanted off the clear urine, leaving the sediment and crust alone, and filled up the glass with lime-water: upon which the sediment immediately rose from the bottom, lost its colour, and the mixture became white and turbid; the crust on the sides of the glass quickly disappeared; and in a short time a large light white sediment fell to the bottom, which, though allowed to stand thirty hours, did not in the least adhere to, or leave any crust upon the bottom or sides of the glass.

HAVING poured off what was clear, I added some white-wine vinegar to this sediment; upon which it immediately disappeared, and the liquor became pretty clear, not unlike sherry; which, however, after some hours, let fall a dark-coloured sediment.

HENCE we see that lime-water has not only a power of hindering the urine from resolving into those principles which are imagined to give rise to the stone, but also of destroying and changing their nature after they are separated from it; whence arises a strong probability, that it may not only hinder the generation of the stone in the human body, but also dissolve it after it is formed: nay, altho' it should be allowed that lime-water does in a great degree lose its dissolving power before it arrives at the bladder; yet, if it shall, by destroying the petrifying quality in the urine, hinder any new accretions to the *calculus*, this must necessarily in time have its surface washed down, and worn away, by the urine continually running along it, and the coats of the bladder acting upon it; as we find even the hardest rocks yield to common water.

Quid magis est faxo durum? quid mollius unda?

Dura tamen molli faxa cavantur aqua. OVID.

BUT

* This, as well as the quantity of white sediment, varies according to the strength of the urine.

BUT that the dissolving quality of the lime is really communicated to the urine, evidently appears from experiments made with the urine of such patients as have taken Mrs Stephens's medicines for a considerable time in large quantities. Thus Mr Morand found, that a pretty hard *calculus* had lost something of its weight, and had its surface corroded, by having daily poured upon it for ten days the fresh urine of a patient who had taken Mrs Stephens's medicines for upwards of a month *. And, if I rightly remember, much the same experiment was tried by Dr Kirkpatrick on his own urine, and with the same success †.

Do not these experiments afford us a clear reason why the stones which Mr Millar passed after drinking the lime-water, were of a whitish colour ‡, whereas all that he had voided, for thirty years before, were brownish? And do they not likewise render it probable, that the great quantity of white sediment in the urine of such people as have taken Mrs Stephens's medicines, has been owing to the action of the lime contained in them? For we find lime-water produce that sediment in urine out of the bladder, and, by drinking lime-water, Mr Millar's urine deposited it in great abundance. The quantity of this, however, will be increased by what is daily washed off the surface of the stone by the efficacy of the medicines.

Does it not likewise appear plain from these experiments, why Dr Jurin's urine || (especially after his largest dose of soap-lees) was whitish and turbid when he first made it, and afterwards deposited a *calcareous* sediment, as he calls it; but which, notwithstanding the opinion that has hitherto prevailed of its being furnished in a good measure by the medicines § seems to have been owing solely to the change made by the lime upon the sediment of the urine.

And

* Memoires de l'Acad. des sciences, an. 1740. edit. 8vo, p. 261. 262. See also the Bishop of Londaff's case in the appendix to this essay.

† See his case written by himself.

‡ Agreeable to this, the ingenious Mr Hay, in his case annexed to Deformity, an essay, tells us, that while he continued to take soap and lime-water, he never discharged any red sand with his urine; but constantly did it when he omitted the use of these medicines, even for a few days.

|| Jurin's case, p. 12.

§ Hales Experim. p. 12.

And that lime-water not only changes the colour of the urine, but of the surface of the *calculus* itself, plainly appeared in a stone (taken from the body of John Greig, who died December 1741, in the Royal Infirmary, of an iliac passion) which had its external surface almost entirely white and a little rotten, while within it was of a sandy colour. Of this no reason could be assigned, but his having drank lime-water for eight days, to the quantity of about an English pint a-day. And it is observable, that as he left off the lime-water eight or ten days before his death, so in some places there was a brownish crust beginning to grow over the white surface.

10. APRIL 15. I put into a phial a scruple of oyster-shell lime and ten drams of fresh urine. Into another, I put the same quantities of salt of tartar and fresh urine: both these mixtures immediately sent forth a vapour which affected the nose with a pungent smell, somewhat like that of spirit of *sal ammoniac*, but more disagreeable. Into a third phial I put equal parts of fresh urine and lime-water, which gave an extremely faint smell of the same kind: And, into a fourth one, the same fresh urine alone. Having corked these several phials, I did not open them till the 16th of May, when the urine with the quick-lime had an extremely offensive smell, not to be described in words. The urine mixed with lime-water had a disagreeable smell of the same kind, but not so strong. The urine with salt of tartar smelled like stale urine, and did not affect the nose near so disagreeably as the two former. The urine without any mixture had a stale smell, but not so strong as that which was mixed with salt of tartar.

HENCE it appears that quick-lime, lime-water, and fixed alkaline salts not only volatilise the salts of the urine, but also corrupt its oils, altho' quick-lime and its water produce this last effect much more remarkably than the alkaline salt.

S E C T. III.

Experiments with lime-water upon the calculus.

IN the following experiments I made use of two *calculi*.

THE first, which, for brevity, I shall denote by A, was given me by my ingenious friend and colleague Dr Monro senior, professor of anatomy in this place. It was of a close texture, very hard, and of a grey sandy colour.

THE second, B, was the one which I just now mentioned to have been taken out of the bladder of John Greig. It seemed fully as hard as the former, and was capable of receiving a pretty fine polish. It weighed an ounce and a half, and its specific gravity was to that of water, nearly as 1704 to 1000. Its colour was pretty much the same with the former.

11. A fragment of A, weighing 23 grains, being put in stone lime-water, and kept in a moderate heat, was mostly all rotten and dissolved in little more than thirty days.

A fragment of B, weighing 10 grains, after two days and nine hours warm digestion in the same lime-water, had two grains of its substance rotten and dissolved.

12. SOME stone lime-water that I made by flaking quick-lime with boiling lime-water, dissolved a piece of A of five grains in about seven days.

13. A fragment of A, gr. vi. after seventeen days cold digestion in stone lime-water, in the month of February, had lost none of its weight, nor was its surface very sensibly softened, altho' it had somewhat of a rotten appearance; while a piece of B, of 12 grains, in six days cold maceration in the end of May, had two and a half grains of its substance dissolved.

FROM this experiment, together with N^o 20. and 57. below, one may be able to account for Dr Lobb's having found lime-water to have no power of dissolving the stone *: for if the lime-water was but

* Treatise of dissolvents of the stone, p. 326.

but weak *, and if the experiment was made in an open vessel, and in the winter-season, it is no great wonder, if, even after a cold maceration of twelve weeks, there was no appearance of dissolution upon the stone.

It is observable, that although the chymists seem generally to have been of opinion, that there lay concealed in quick-lime a powerful remedy against the stone, which by their art might be extracted out of it; yet none of these authors, that I have met with, among the many diseases for which they have highly commended lime-water, so much as hint at its being serviceable in the stone or gravel; in which diseases, however, its virtue is perhaps more remarkable than in any other. And one may almost venture to affirm, notwithstanding the boastings of many chymical writers, of their spirit of quick-lime, and other secret preparations of it, that its virtues are more easily extracted, and more safely conveyed into the blood by its water, than any way else.

HAVING thus found a considerable power of dissolving the *calculus* in stone lime-water, I thought it might be worth while to inquire, whether shell-lime possessed the same virtue in a greater or less degree.

14. A piece of A, weighing nine grains, was dissolved by lying seventeen days in lime-water made with calcined egg-shells, in a digesting warmth; and this, I am apt to think, might have happened somewhat sooner, had the shells been more carefully calcined.

15. A fragment of A of six grains was, by two days warm digestion in lime-water made with oyster-shells, reduced to two grains, and in three days to less than one grain.

16. A piece of B, eight grains, in thirty-six hours warm digestion in oyster-shell lime-water, had about three grains and one fourth of its substance dissolved.

17. HAVING calcined a few cockle-shells, that had lain long exposed to the weather, I put a piece of B, eight grains, in some of the lime-water made with them; which, in thirty-six hours warm digestion, lost near three grains and a half.

THE

* That this was the case, is highly probable, since he made his water with unflaked lime, which he, in the next paragraph, tells us was lime a little flaked.

THE lime-water, especially that from shells, generally dissolves the stone by making it throw off white rotten crusts or scales; which, if allowed to lie long enough among the water, and if the glass be shaken now and then, are reduced to a sort of white mucilage, resembling in some degree the white sediment of N^o 9.; but which, when dried, has the appearance of fine powdered chalk. This serves farther to shew that the white sediment in the urine of such as drink lime-water, is not derived from the lime, but wholly from the parts of the stone, and grosser parts of the urine, thus changed by the lime-water*.

18. A piece of A, six grains, was rendered pretty soft, and entirely rotten, by a cold maceration of seventeen days in oyster-shell lime-water, in the month of February; but May 19. having infused, cold, a fragment of B, eleven grains, in some of the same lime-water, it lost in three days near five grains, and in eight days was reduced to a *nucleus*, weighing three grains.

HERE it may be proper to observe, that, unless the *calculus* be small, and the quantity of lime-water in which it is immersed be large, the water must be renewed at different times, since its virtues are weakened in proportion to the quantity of the stone which it has dissolved; and, as far as I have been able to observe, less than half a dram of *calculus* reduced to a fine powder, is sufficient to destroy the virtue of two ounces of strong lime-water in which it is infused.

FROM these experiments it appears, that oyster and cockle-shell lime-water possesses a much greater power of dissolving the *calculus*, than that of stone-lime.

ALTHOUGH, as has been already hinted, I don't find that lime-water has been perscribed till of late in the gravel, or for dissolving the stone in the bladder; yet, since the first publication of these papers, I have met with a passage in an epistle of Olaus Borrichius to Thomas Bartholine, by which it plainly appears, that the power of shell lime-water to dissolve the *calculus* out of the bladder, was long ago known to this author. His words are, "Constat autori

Y y

"tate

* See No. 9. above.

“tate Bafilii Valentini aliorumque, nihil in calculo profligando
 “utilius spiritu calcis vivæ; mihiq; iterum iterumque comper-
 “tum, aquam calcis vivæ ostreorum, mytilorumque, solvere calcu-
 “los ordinarie ab ægris exfectos in mucilaginem, si aliquot dierum
 “leni fotu in calido simul detineantur *.”

19. WITH regard to the quantity of water to be poured on quick-lime; it may be proper to observe, that the difference of strength in the lime-water will be very inconsiderable, whether 8, 10, or 12 times its weight of water be added to the quick-lime, provided the water remains long enough on it to be fully impregnated with its virtues. But I have observed, that after the shells have been but 24 hours from the fire, they neither make such an ebullition with water, nor impregnate it so quickly with their virtues.

IF cold water be poured upon shell-lime, very little heat or ebullition ensues; yet the lime-water thus procured seems to have as great a power of dissolving the stone, as when made with boiling water; but is more harsh and disagreeable, the other having a softness and sweetness which this wants.

THE water, whether cold or hot, should be allowed to stand at least eighteen hours on the fresh calcined shells; or longer, if the quantity of water added to them is in a greater proportion than eight or ten to one.

THE shells will calcine in any fire, provided it be hot enough; and the cockle and oyster with much less trouble than the egg-shells. If they are friable, and quite white, they are sufficiently burnt; but if blackish or grey, they must be put into the fire again.

IT may perhaps be worth while to observe, that if any part of the shells is blueish and not properly calcined, the water poured on them will get a very disagreeable sulphureous taste.

IF any one before reading this essay should chance to have consulted Mr Lymery upon lime-water, either in his chymistry or *Pharmacopée universelle*, he will perhaps be surpris'd to find three or four English pints of this ordered to be taken every day, while that learned chymist talks of its dose, as from one to four ounces; speaks of its

* Bartholin Epist. centur. 4. epist. 79.

its raising great thirst, and being in danger of burning the stomach; upon which account he orders syrup of violets to be mixed with it, and prefers the second water to the first. Perhaps from such authority it partly was, that many physicians have been used to prescribe this medicine with so sparing a hand; and I own, that first when I began to order lime-water for the stone, I was afraid to exceed an English pint of it a-day; but, as repeated experience has taught me, that there is nothing to be dreaded from it when taken in much larger quantities, I imagine Mr Lymery's suspicions of its doing mischief in large doses, or when taken unmixed with any thing to soften it, were not so much owing to any experience of its bad effects, as to theory. Quick-lime is remarkably corrosive, and has been believed to act by its fire; therefore it was natural to think, that lime-water impregnated with the same fire, might have the same effect, though in a less degree.

20. I have made experiments with lime-water and several other urinary stones, but have never yet met with any that were able to resist the oyster or cockle-shell lime-water, although some that were extremely hard, and of a dark brown colour, dissolved a good deal slower than either A or B.

I had indeed sent me a few small stones, all of them of a particular shape, resembling pretty much the stone of a raisin, perfectly smooth, and variegated something like a polished pebble; to which their exterior *cortex* scarcely yielded in hardness. Upon these the lime-water had no manner of influence. But if these stones came from the urinary passages, as I was told, they were of a texture quite different from all the gravel-stones I have ever seen; and their being all pretty much of one bulk and shape, makes it probable that they were generated in some particular cavity. In the Philosophical transactions * we have the history of a woman in Switzerland, who passed by the *anus* great quantities of stones like flint. And I have seen a concretion passed by stool, longer, but not so thick as a hen's egg, with its exterior *cortex* as smooth as a polished pebble, though internally it was of a spongy fungous consistence. In the

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same

* Lowthorp's Abridgment. Vol. 3. p. 167.

same Transactions we read of a shell having been found in one of the kidneys of a lady who had been often subject to violent vomitings *; which it is very likely the lime-water would not have dissolved. And Bartholine mentions stones said to have been extracted from the bladder, of a flinty hardness †; though Olaus Borrichius seems to doubt of this ‡. But as these concretions are as different in their nature from common *calculi*, as flint or shells are from free-stone, and besides are extraordinary and very rare, lime-water (especially that from oyster or cockle shells) may be still looked upon as a pretty universal dissolvent of calculous concretions.

As the calculous concretions of the urine have been thought to have some analogy to the tartarous crust left by wine upon the sides of the cask, it may perhaps be worth while to observe, that lime-water dissolves tartar pretty quickly; but as its virtue is soon destroyed by the acidity of the tartar, the affusion of fresh lime-water must be frequently repeated.

21. THE Reverend and ingenious Dr Stephen Hales having wrote me (May 1751) that he had found oyster-shell lime-water, made with a pound of lime to an English gallon of water, rendered sensibly more pungent to the taste, as well as a more powerful solvent of the stone, by pouring it on calcined shells red hot from the fire; I made the following experiments with a view to determine with some precision the different strength of different lime-waters.

(a) JUNE 6. At nine in the evening, I poured upon a pound of calcined oyster-shells hot from the fire, seven pounds of boiling water.

(b) NEXT day, at eight in the evening, I poured two pounds of this lime-water upon half a pound of calcined oyster-shells newly taken from the fire, and still warm. Thirteen hours after this, I decanted off and filtered fourteen ounces of each of these waters.

(c) AT the same time I filtered the like quantity of lime-water, which was procured by pouring, forty-eight hours before, seven pounds of boiling water upon a pound of calcined oyster shells, which, in the space of four days and a half, had formerly got three such waters.

(d) I

* Lowthorp's Abridgment. Vol. 3. p. 162.

† Epist. 45. cent. 4.

‡ Barthol. Epist. 76. cent. 4.

(d) I filtered also fourteen ounces of lime-water, made by pouring seven pounds of boiling water upon one pound of stone-lime, taken a few hours before from the fire.

IN order to find the specific gravities of these different lime-waters (the first of which we shall, for brevity's sake, call A, the second B, the third C, and the 4th D), I weighed, with the assistance of my colleague Dr John Stuart professor of natural philosophy, a large glass-phial (filled with sand, and hermetically sealed) first in air, and then immersed it in the fountain-water of this city (used also in making the several lime-waters) by which it lost 3704 grains of its former weight. Being next weighed in B, it lost 3727 grains. In A it lost 3720 gr.; in C 3710 gr.; and in D 3713. Hence it appears that the specific gravity of the double lime-water B, was to that of Edinburgh water, near as 169 to 168. The gravity of A was to that of the same water, nearly as 232 to 231; of C, nearly as 617 to 616; and of D, nearly as 411 to 410. At another time I found, by the same method, the specific gravity of some lime-water procured from oyster-shells; upon which, during the course of twelve months, I had at different times poured, at least, a hundred times their weight of water, to be to that of fountain-water, nearly as 926 to 925.

It may be proper to observe, that both A and B were made with oyster-shells, which had lain buried in the rubbish on the south-side of the castle of Edinburgh, probably above a hundred years; because, if they had been newly got from the sea, the salt which they contain, even after calcination, would probably have increased the specific gravity of the waters poured on them, and so have rendered the experiments less accurate.

THE tastes of A, B, C, and D, as well as their specific gravities, shewed their different strengths. A and B had much the same kind of taste, but B was sensibly more pungent; C had much less pungency than either of the two former, and left a sweetishness in the mouth, somewhat resembling the taste of liquorice-root; D did not differ much from C as to its taste.

I put three pieces of a very hard *calculus*, each weighing ten grains, into

into three different phials; one of which I filled with the lime-water A, another with B, and the third with C. After they had stood in the same degree of warmth 93 hours, I found that the *calculus* in the double lime-water B, had two grains of its substance dissolved or rotten; the *calculus* in A had lost somewhat more than a grain and a half; and that in C, a grain.

HENCE it follows, that water does not acquire the same precise degree of strength, whether five or five hundred times its weight of water be poured on quick-lime; but that, strictly speaking, lime-water is stronger or weaker as a greater or less quantity of quick-lime is added to the water; that where the proportions of these two are equal, fresh calcined shells impregnate water more strongly than such as have had several affusions before; and that lime-water, made with calcined shells, hot from the fire, may be rendered stronger, by pouring it a second time on fresh-burnt shells. It is, however, observable, that this double lime-water, notwithstanding it remains on the shells, loses in a few days part of its pungency and strength, while the weaker lime-waters seem to be constantly supplied with fresh virtue from the shells, sufficient to make up what they lose by the contact of the air.

FARTHER, we see that stone-lime does not impregnate water so strongly with its virtues as shell-lime. Whether this may be owing to the greater subtilty of the latter, whereby it more easily mixes with, and is suspended in the water, or whether it may not proceed from some other cause, I shall not presume to determine. However, from the greater specific gravity of shell lime-water, we are enabled to account for its dissolving the *calculus* more powerfully than stone lime-water.

BUT altho', from the above experiments, it evidently appears, that fresh calcined shells afford a stronger water than such as have been exposed for any considerable time to the air, and that the first water poured on quick-lime is stronger than the succeeding ones; yet it is far from being true, that quick-lime is soon deprived of its virtues by repeated affusions of water, or that the third or fourth water procured from it is altogether insipid, as some have affirmed.

ed*. 'Tis true, indeed, that fresh calcined shells impregnate water with their virtues sooner, as well as in a greater degree, than shells which have been in some measure flaked by long exposition to the air; and that when repeated waters are poured upon quick-lime, it communicates its virtues not only more slowly, but also, strictly speaking, in a less degree, to each succeeding water †: but nevertheless, quick-lime is so far from having its virtues wholly extracted by a few affusions of water, that some stone-lime, which had been reduced to a powder, by being exposed to the open air for four months, still retained some of its virtue, after having had, during the course of sixty days, 260 times its weight of water poured upon it: and some lime-water procured from calcined oyster-shells, upon which in 48 days, I had poured 270 times their weight of water, was, by experiment, found to be possessed of a considerable power of dissolving the stone.

HENCE we may see why lime, which has been used since the times of the Romans ‡ as a manure, retains its power of fructifying the ground for many years.

It

* Memoires de l'Acad. des sciences. 1700. edit. 8vo, p. 160. and 170. (a).

(a) It seems to have been the common opinion, that the first and second waters got off quick-lime, are not only the strongest, but that the third, fourth, and succeeding ones are almost insipid and destitute of any virtue. But Dr Alston has lately informed us, that quick-lime not only continues to communicate its virtues to water much longer than any one had imagined before, but also affirms, that the last water is as strongly impregnated with the virtues of the lime as the first; Philosoph. transact. v. 47. p. 266.; and Dissertation on quick-lime, &c. p. 4. 5. and 6. In this case, however, as in many others, the truth is wholly on neither side; for although quick-lime does communicate its virtues to water much longer than any one could have expected before making the trial, yet it is most certain, that the waters got off quick-lime become gradually weaker, and that the first water contains more of the lime, and is stronger, more pungent, and disagreeable to the taste, than the 10th, 12th, or succeeding ones. The discovery, therefore, of which the Doctor claims the honour (b), and indeed justly, comes out to be partly real, and partly imaginary: and the reason why the author of this essay took no particular notice of this discovery, in the former edition, was the aversion he had to criticise upon his good friend, or to enter into any controversy with him.

† Calcined shells, taken fresh from the fire, will, in less than 24 hours, communicate all the virtue they can give to seven or eight times their weight of water; but after they have had twelve or fourteen waters poured on them, several days must pass before the water gets all the strength which the lime can give it.

(b) Dissertation on quick-lime, p. 47.

‡ Hedui et pictones calce uberrimos fecere agros; quae sane et oleis, et vitibus utilissima reperitur. Plin. Histor. natural. lib. 17. cap. 8.

It follows also, from the above experiments and observations, that calculous patients may not only use, with advantage, the first, but also several more waters procured from quick-lime: and it may sometimes be adviseable for them to begin with the 3d and 4th waters, and use them for some days before they try the first water, which has a more pungent and disagreeable taste.

My worthy colleague Dr Alston, being neither satisfied with the above experiments, concerning the strength of different lime-waters, nor with the conclusions drawn from them, has, in the first edition of his Dissertation on quick-lime, endeavoured to destroy their credit, by arguments chiefly taken from the imperfection of the hydrostatical balance, and from the supposed nature of quick-lime and its water. These I should have passed over in silence; but as some of the Doctor's experiments came out different from mine, in order to know whether I might not have been mistaken in what I had formerly advanced, I thought it necessary to make some new trials*; which have given occasion to very ample remarks†, but have equally failed of satisfying my friend as the former; for he still maintains, that lime continues (till it be exhausted of its virtues) to impregnate water as strongly as when first taken from the fire; that shell-lime water is not stronger than that which is made with stone-lime, and that lime-water cannot be made stronger by pouring it a second time on fresh lime.

To answer all the Doctor's objections to my experiments, and his arguments in favour of his own notions, would be to me an unpleasant task, and afford to the reader but disagreeable entertainment: I shall therefore content myself with relating a few simple and easy experiments, which, far from throwing dust upon and darkening, serve to decide the matter in dispute.

I. (1.) JUNE 20. 1754, I poured upon four ounces of oyster-shell lime, reduced to a gross powder, rather more than eight times its weight of water. After 24 hours, I filtered 12 ounces and one dram

* Vid. Edinburgh Physical essays, vol. 1. art. 13.

† Dissertation on quick-lime, 2d edit. p. 58. &c.

dram averdupois weight of this lime-water through brown paper, and added to it 30 grains of falt of tartar : the white powder which fell to the bottom, being separated from the water by filtration, and well dried, weighed full 13 grains.

(2.) AFTER this, in the course of seven days, I poured upon the same oister-shell lime a 2d, 3d, 4th, 5th, and 6th time, the same quantity of water ; after the sixth water had stood thirty-six hours on the lime, I filtered it, and added 30 grains of falt of tartar to 12 ounces and one dram of it. The white powder which fell to the bottom, being well dried, weighed 10 grains.

(3.) AFTERWARDS, *viz.* from June 28. to July 6. I poured upon the above lime the same quantity of water a 7th, 8th, 9th, and 10th time. The tenth water having been filtered after it had stood on the lime three days, 12 ounces and one dram of it mixed with 30 grains of falt of tartar, afforded of white powder $8\frac{1}{2}$ grains.

(4.) FROM July 6. to August 7. I poured on the same lime seven waters more ; the last of which, being the 17th, was not filtered till it had stood on the lime seven days. Twelve ounces and a dram of it, mixed with 30 grains of falt of tartar, yielded 7 grains of white powder.

II. (1.) JULY 3. I poured upon four ounces of stone-lime, about twenty hours after it had been taken from the kiln, near nine times its weight of water. After 24 hours, I decanted off and filtered the water. Twelve ounces and one dram of it, mixed with 30 gr. of falt of tartar, afforded scarcely 10 grains of a white powder.

(2.) AFTERWARDS, in the space of twenty-one days, I poured on this stone-lime nine waters more ; the last of which being the 10th, was allowed to remain on the lime three days. Twelve ounces and one dram of it, mixed with 30 grains of falt of tartar, gave $8\frac{1}{2}$ grains of white powder.

(3.) AFTER this, in the course of 41 days, I poured on the same stone-lime seven waters more ; the last of which, being the 17th affusion, was not filtered till it had stood full seven days on the
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lime. Twelve ounces and one dram of it, mixed with 30 grains of salt of tartar, gave $7\frac{1}{3}$ grains of white powder.

III. (1.) THE above lime-waters were all made in glazed earthen vessels, the mouths of which were not covered; this being the common way of making lime-water, and that which both Dr Alston * and I generally observed. But that I might know what difference would be produced in the strength of the water by infusing the lime in a close vessel, I put three ounces of oyster-shell lime into a glass-bottle, adding to it 27 ounces of water. Having closely corked the bottle, I let it stand seven days (shaking it, however, once every day); twelve ounces and one dram of it being filtered and mixed with 30 grains of salt of tartar, gave $15\frac{1}{2}$ grains of white powder.

(2.) AT the same time, I put into another bottle three ounces of stone-lime, with 27 ounces of water. After an infusion of seven days, twelve ounces and one dram of this water, mixed with 30 grains of salt of tartar, afforded $11\frac{3}{4}$ grains of white powder.

(3.) I put also into a crystal bottle, with about eight times its weight of water, the oyster-shell lime that had got 17 waters before. Having close-stopt the bottle, and let it stand seven days, I filtered the water, and found that twelve ounces and one dram of it, mixed with 30 grains of salt of tartar, gave $9\frac{1}{3}$ grains of white powder.

THE same quantity of the 18th infusion, in a close-stopt bottle, of the stone-lime (II.), afforded $9\frac{1}{2}$ grains of white powder.

(4.) I infused 20 grains of stone-lime, in 25 ounces or 600 times its weight of water, in a corked bottle, for seven days; after which, 12 ounces and one dram of the water, filtered and mixed with 30 grains of salt of tartar, gave near 9 grains of white powder.

THE same proportions of stone-lime and water being infused for the same time in an open vessel, 12 ounces and one dram of the water afforded, with the same quantity of salt of tartar, only $3\frac{1}{4}$ grains of white powder.

(5.)

* The experiments in the Dissertation on quick-lime, (1st edit.) p. 4. 5. 6. and 7. which first led Dr Alston to think there was no difference between the 1st and 20th lime-waters, were all made in open vessels.

(5.) TWENTY grains of oister-shell lime, infused, in a close-stopt bottle, in 600 times its weight of water, after seven days, gave a lime-water, 12 ounces and one dram of which afforded $8\frac{1}{2}$ grains of white powder.

(6.) THIRTY grains of oister-shell lime being infused in 400 times its weight of water, in a close-stopt bottle, for seven days, afforded a lime-water, twelve ounces and one dram of which gave 10 grains of white powder.

THE same proportions of oister-shell lime and water having stood during six days in an open vessel, 12 ounces and one dram of the water gave 4 grains.

SINCE the quantity of calcarious powder, precipitated from mixing lime-water and salt of tartar, must be in proportion to the strength of the lime-water, whether this powder be supposed to arise from the salt or from the water, or from both *, it must follow from the above experiments,

1. THAT quick-lime communicates more of its virtues to water at first, than afterwards; and although the difference between the first and second waters may be so small as scarcely to be discoverable by our senses; yet the sixth is manifestly weaker than the first; the tenth is weaker than the sixth; and the seventeenth is weaker than the tenth. It is, however, observable, that this decrease of strength is greater in oister-shell than in stone-lime water.

2. IT appears, that when oister-shell lime has 400, and stone-lime has 600 times its weight of water poured on it †, less virtue is communicated to the water, than when the proportion of the water to the lime is as 8 or 9 to 1. And this holds true, whether the infusion be made in open or close vessels.

3. ALTHOUGH oister-shell lime did not impregnate 600 times its weight of water quite so much as stone-lime, which may be owing, perhaps, to its not having been perfectly calcined; and altho' stone-lime impregnates water rather more strongly than oister-shell lime, after they have had 16 or 17 waters poured on them; yet it is evi-

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dent,

* It will be shown in sect. 9. below, that the precipitation is almost wholly from the lime-water.

† Dr Alston's Dissertation on quick-lime, p. 6.

dent, that oyster-shell lime communicates, at first, a good deal more virtue to water than stone-lime; and this seems to be the reason why it is much more weakened by the first ten infusions than the stone-lime.

DR ALSTON has called in question the superior virtue of oyster-shell lime-water; but, in many repeated experiments which I have made upon *calculi*, this and cockle-shell lime-water proved always the strongest dissolvents: My ingenious friend Dr Home has observed, that shell-lime water corrodes linen faster than stone-lime water: and Mess. Du Hamel and Grosse found, near 30 years ago, that a greater quantity of tartar was dissolved by oyster-shell lime-water than by that made with stone-lime*.

4. IT is so far from being true, as Dr Alston imagines †, that lime, as long as it retains any virtue, will impregnate water with as much of its finer part as it can receive; that quick-lime does not give, even to the first water, when made in an open vessel, so much of this as the water can take; since the water may be rendered sensibly stronger, by making it in a close-stopt bottle. And, it is worth remarking, that the difference of the quantity of calcarious powder, afforded by the 1st and 17th infusions of oyster-shell lime in open vessels, and by the 1st and 18th infusions of it in close-stopt bottles, was nearly the same, *viz.* about 6 grains. But when a very small quantity of quick-lime was infused in a great deal of water, the difference of strength between the infusions in close and open vessels was greater, because, during the seven days infusion, the water in the open vessel lost a great deal of its virtue, which the lime could not supply; while in the close-stopt bottle, the water was always getting something from the lime, and, in the mean time, lost little or nothing.

5. If lime-water, made in an open vessel, be weaker than when it is made in a close one, why may not single lime-water acquire some more strength by the addition of fresh lime, since it is manifest it can take up more of the virtue of the lime than it had before? It is probable,

* *Memoir de l'Acad. des sciences.* an. 1732, edit. 8vo, p. 454.

† *Dissertation on quick-lime*, p. 54.

probable, however, that when lime-water is made in close-stopt bottles, the addition of fresh lime to it will not make near so great a difference of strength in it, as when the infusion is made in open vessels *.

6. THAT lime-water made in open vessels, may acquire more strength by having fresh lime added to it, the experiments related above (p. 357.) with those in the Physical and literary essays †, seem to demonstrate. Nay, Dr Alston himself owns, that having weighed some quadruple lime-water (which, by the bye, from the small proportion of lime added each time to the water, should not have been so strong as our double lime-water (b,) p. 356. above,) in a flask, which contained 8940 gr. of single lime-water, he found its weight to be 8947 grains ‡. But though the Doctor is, at last, convinced by the force of his own experiments, that double lime-water may be heavier than single; yet he will not allow even this very small excess of gravity in his quadruple lime-water to be owing to its containing more of the lime than single lime-water, but to some heterogeneous substances dissolved in it. And this he thinks he has proved, by finding that the flask filled with this quadruple lime-water, after it had thrown up its crusts, exceeded the same flask filled with common water by four grains.

EVEN this experiment, supposing it were liable to no objection, accounts only for about one half of the difference of weight between the Doctor's single and quadruple lime-water. But if we consider the thing more accurately, we shall see that no great stress can

* I infused, in a close-stopt bottle, 3 ounces of oyster-shell lime, in 27 ounces of water; after 12 hours I added 6 ounces more of the same lime fresh from the fire, and in less than two days filtered the water; 12 ounces and 1 dram of which gave, with 30 grains of salt of tartar, very near 16 grains of white powder. But, as the water tasted very strong of the lime, I added to it 15 grains more of the salt of tartar, and had, by filtration, 1 grain of calcarious powder; so that this double lime-water afforded near 17 grains of earth. Let it not be said here, that the grain of white powder got by the addition of the 15 grains of salt of tartar, proceeded from the salt; since, when that quantity of this salt is dissolved in common water, the precipitation does not amount to $\frac{1}{7}$ of a grain. Whether this double lime-water might not have acquired yet more strength, had it stood longer on the lime, I cannot say, not having had leisure to repeat the experiment.

† Vol. 1. art. 13.

‡ Dissertation on quick-lime, 2d edit. p. 61.

can be laid upon it. For, was the Doctor sure this last water was quite effete? I found, that after nineteen days exposition to the open air, twelve ounces of strong lime-water, upon being mixed with eight grains of salt of tartar, afforded $1\frac{1}{2}$ grains of calcarious matter, when 30 grains of the same salt dissolved in common water did not afford the $\frac{1}{6}$ part of that quantity. Is it not, therefore, more than probable, that the four grains, by which the effete quadruple lime-water exceeded common water in the Doctor's experiments, were partly, if not wholly, owing to its not being intirely deprived of its lime? and this the rather, that he only exposed this lime-water ten days to the open air *.

FARTHER, when the difference of specific gravities amounts only to a few grains, one cannot safely rely on such a machine as the Doctor used; and to set it up in opposition to the hydrostatical balance, would be much the same as if one should contend that he could describe a circle as accurately without compasses as with them.

I shall only add, that double lime-water not only appeared, by its taste and effects on the stone, stronger to Dr Hales than single lime-water, but, by its weight, mixture with claret, &c. convinced me also of its superior strength: and Dr Home has found, that lime-water acquired a greater degree of hardness by being poured a second or third time upon quick-lime. So that, upon the whole, if I be mistaken in thinking that lime-water may be rendered stronger by adding fresh lime to it, I shall at least have the excuse of not being singular in my mistake.

TABLE

* What is here suggested may shew, why Dr Allston did not find a sensibly greater quantity of crusts in double than in single lime-water; and that the strength of lime-waters cannot be exactly determined by the quantity of crusts which they throw up, unless the water is either evaporated, or allowed to stand till it gives no more precipitation with salt of tartar than common water; neither of which methods were observed by the Doctor in his experiments.

TABLE of the strength of different lime-waters according to the foregoing experiments.

OISTER SHELL LIME-WATER made				In close Vessels.			
In open vessels.							
Infusions.			Strength.	Infusions.			Strength.
First	-	-	13	First.	-	-	$15\frac{1}{2}$
Sixth	-	-	10	Eighteenth	-	-	$9\frac{1}{8}$
Tenth	-	-	$8\frac{1}{2}$				
Seventeenth	-	-	7	With 400 parts of water			
				to one of quick-lime	10		
With 400 parts of water				With 600 parts of water			
to one of quick-lime	4			to one of quick-lime	$8\frac{1}{2}$		

STONE LIME-WATER made				In close vessels.			
In open vessels.							
Infusions.			Strength.	Infusions.			Strength.
First	-	-	10	First	-	-	$11\frac{3}{4}$
Tenth	-	-	$8\frac{1}{2}$	Eighteenth	-	-	$9\frac{1}{2}$
Seventeen	-	-	$7\frac{1}{5}$				
With 600 parts of water				With 600 parts of water			
to one of quick-lime	$3\frac{1}{4}$			to one of quick-lime	9		

S E C T. IV.

Experiments with lime-water and some of the animal humours.

THE great power of lime-water to dissolve the *calculus* being sufficiently made out in the above experiments, the next subject of inquiry seemed to be, How far the nature of lime-water would be changed by its being mixed with the humours of our body, and consequently what probability there was of its carrying its virtue along with it to the bladder.

22. I infused a piece of B, three grains, in a mixture of *saliva* and oister lime-water, in the proportion of one of the former to two and a half of the latter: Its surface in a few hours began to turn white; and, upon shaking the glass, threw off white scales; and, in two days warm digestion, it was reduced to a grain and a half.

23. I immersed another fragment of B, three grains, in an ounce of *cystic bile*, and three ounces of oister lime-water, which being kept in a moderate heat forty-two hours, had near one grain and a half of its substance dissolved in the form of thin whitish scales.

[23.] A piece of B, five grains, being infused in one part serum of human blood, and three and half a parts oister lime-water, was, after thirty-eight hours warm, and sixteen cold digestion, reduced to three grains.

24. I put also a piece of B, five grains, in a mixture of one ounce of fresh urine and three ounces of oister lime-water: After digesting three days in the same heat with the last, its surface was become all over white, about a grain of it was dissolved, and the rest somewhat rotten and friable.

SINCE, from these experiments, it appears, that the animal humours have nothing in their nature peculiarly destructive of the dissolving quality of lime-water, we might reasonably conclude, *a priori*, that it should carry its virtue along with it even to the bladder, and so in time dissolve the stone.

S E C T. V.

Experiments with lime-water, and fermented liquors and spirits.

HAVING shewn the probability there is that the animal fluids will not destroy the virtue of lime-water, we come next to inquire, How far it may be affected by such liquors as are most commonly used in this country.

25. CLARET wine destroys the taste of near double its quantity of lime-water, and the liquor has the colour of the wine rather heightened,

heightened, and the taste of wine and water ; but, upon adding a little more lime-water, it acquires a blackish colour, not unlike gun-powder, and begins to discover a little of the taste of the lime. The quantity required to produce this change of colour in the claret varies according to the strength of the lime-water. Having once flaked some quick-lime with boiling lime-water, I found that the water made in this way turned claret black when mixed with it in the proportion of one and a half to one.

I took two pieces of A, each weighing twenty-three grains : The one being put in lime-water, and kept in a moderate heat, in five days had near five grains rotten and dissolved ; the other being put in one pint claret and two pints of the same lime-water, and kept in the same heat for fifteen days, lost nothing of its weight, nor was its surface at all softened.

25. AN ounce of lime-water mixed with an equal quantity of strong-ale, not in any degree stale, having been but fourteen days in the bottle, had its taste quite destroyed, at the same time that it weakened the malty taste of the ale more than an equal quantity of common water. A piece of A, weighing ten grains, after lying thirteen days in this mixture in a moderate heat, had lost nothing of its weight, nor were there any signs of dissolution about it.

SMALL bear has the same effect, but in a less degree.

27. UPON mixing lime-water and vinegar, there does not arise any effervescence or ebullition ; one ounce of the latter destroys the taste of ten or twelve of the former ; and a fragment of A, that weighed four grains, after lying eight days among this saturated mixture in a moderate heat, had lost nothing, nor was it any way softened.

HENCE it seems proper for such as use lime-water, to abstain not only from all acids, but also from wine, ale, and, so far as I have been able to observe, all fermented liquors. And doubtless Mrs Stephens, by ordering her powder to be taken in a tea-cupful of white wine, cyder, or small punch, must have greatly impaired the strength of it, and rendered it much less effectual (although less offensive to the stomach) than otherways it would have been.

28. A spoonful of West-India rum, mixed with the same quantity of lime-water, produces a liquor of a fine limon-colour, both tasting and smelling strong of the lime. A little vinegar added to this immediately changes its colour, and destroys all taste of the lime.

RUM, in which so much limon-peel had been infused as to give it a yellow colour, being mixed with an equal quantity of lime-water, acquired a stronger yellow colour; but immediately became turbid; which was owing, I suppose, to the change made by the action of the lime-water upon that oil with which the limon-skins abound, and to which the yellow colour of the rum was owing.

29. EQUAL parts of French brandy and lime-water produce a liquor higher coloured than the brandy was before, but tasting strong of lime. In an hour or two there falls to the bottom a brown sediment, (especially if the brandy was high coloured), and the liquor above becomes of a limon-colour, not tasting of lime; but when the sediment is stirred up, it tastes as before.

MUCH the same thing happens to malt-spirits and rum, when mixed with lime-water; and in proportion as these spirits are more or less free of colour, so is the sediment that falls to the bottom. From this we see, that though these spirits do not destroy the taste of lime-water, yet they have a power of making the lime precipitate in a short time, which, when it falls to the bottom, carries along with it whatever is mixed with these spirits in order to colour them.

30. HAVING infused a piece of B, two grains, in one part malt-spirits, and two parts oyster lime-water, in thirty-five hours warm digestion its surface was become white, and it had about one third of a grain of its substance dissolved.

FROM these experiments we may conclude, that if such persons as drink lime-water cannot confine themselves to watery liquors, it will be better to allow them a little weak punch made without acids, than wine, ale, or any fermented liquors.

S E C T. VI.

Experiments with lime-water and animal food, also milk, honey, and sugar.

31. **I** PUT a piece of B, fix grains, in a mixture of one part mutton-broath and two parts oister lime-water, which, after three days warm digestion, had two grains of its substance dissolved.

32. AT the same time having added half an ounce of a strong decoction of fresh cod-fish to an ounce and a half of oister lime-water, I immersed in it a fragment of B, four grains, which, in the same heat, was in three days and twelve hours reduced to one grain.

HENCE we may conclude, that animal food may be allowed to such as are under a course of lime-water for the stone.

33. A fragment of B, near five grains, being put in a mixture of one ounce of milk and four ounces of oister lime-water, after digesting forty-two hours in a heat rather greater than that of the human body, had some of its substance dissolved under the appearance of white scales, and the greatest part of it was become rotten, so as to crumble down upon pressing it with one's nails.

34. HAVING dissolved two drams of honey in three ounces of oister lime-water, I put into it a piece of B, five grains, fifty-six hours in a moderate heat, it lost only one grain of its weight, and what remained was as hard as ever.

35. I immersed a piece of B, five grains, in three ounces of oister lime-water, in which was dissolved two drams of white sugar; in forty-eight hours warm digestion, it was reduced to three grains; and what remained did not seem quite so hard.

THUS it appears, that honey destroys, in a good measure, the dissolving virtue of lime-water, while the same quantity of sugar weakens it but very little.

S E C T. VII.

Experiments with lime-water and several fruits, herbs, and roots.

THAT such a diet may be ordered for calculous patients as will least destroy the virtue of lime-water, after the experiments upon animal food, we shall relate the effects of different vegetable substances upon it.

36. I infused a fragment of B, eight grains, in half an ounce of juice of strawberries, and two ounces and a half of oister lime-water; after four days warm digestion, and seven days cold, it had lost none of its weight, nor was there any appearance of dissolution about it.

37. I digested a piece of B, six grains, in half an ounce of juice of cherries and three ounces of oister lime-water, six days warm; but its surface was neither softened, nor its weight diminished.

38. A fragment of B, six grains, being put in a mixture of one ounce of a strong decoction of raisins and three ounces of oister lime-water, was not any way changed by three days warm digestion.

FROM these experiments we may infer, that all fruits which have any acidity or sharpness, whether fresh, as gooseberries, strawberries, cherries, apples, pears, plumbs, peaches, &c. or dried, as raisins, prunes, currants, &c. ought to be abstained from by such as use lime-water with a view to the dissolution of the stone.

39. I infused a fragment of B, five grains, in one ounce of a decoction of asparagus and two ounces of oister lime-water; in a few hours its surface began to turn white, and, in thirty-six hours warm digestion, it had thrown off, in white scales, a full grain of its weight. As the grosser parts of the asparagus fell always to the bottom of the glass, it was necessary to keep the *calculus* suspended in the middle of the mixture, by means of a thread, otherways the dissolution does not succeed quite so well.

ARTICHOKES

ARTICHOKES seem to destroy the virtue of lime-water a little more than asparagus.

40. A piece of B, nine grains, by digesting warm, four days, in one ounce of a decoction of turnip, and two ounces of oister lime-water, lost more than a grain.

41. A fragment of B, three grains, being put into a mixture of a decoction of parfly and lime-water, in the above proportion, in three days warm digestion was reduced to one and on fourth grain, having thrown off the rest in whitish scales.

42. In an ounce of decoction of onions, and two ounces of oister lime-water, a piece of B, of seven grains, lost, by thirty-six hours warm digestion, one grain.

43. JUICE of lettuce mixed with lime-water destroys its virtue rather more than any of the above.

44. A fragment of B, nine grains, in one ounce of a strong decoction of althea root, and two ounces of oister lime-water, had, by two days and eighteen hours warm digestion, about a grain of its substance dissolved, and a good part of the rest rotten and friable.

45. I put a piece of B, fourteen grains, into oister lime-water, in which some juniper-berries had been infused; which, in two days and a half, had above two grains dissolved.

GREEN and Bohea tea infused in the same manner do not considerably destroy the virtue of lime-water.

I might have tried the effect of a great many more vegetable substances upon lime-water, had I not been afraid of swelling this essay to too great a bulk; but, from these few experiments, it is probable that most of the following vegetables may be safely used by such as drink lime-water, *viz.* artichokes, asparagus, spinnach, lettuce, fucory, parfly, purslane, onions, leeks, cellary, turnip, carrot, potatoes, radishes, green pease *.

S E C T.

* The juices and decoctions of onions, leeks, and cellary, are observed to have a considerable power of dissolving the softer kind of gravel-stones; and therefore ought to be preferred to most other vegetables for the diet of calculous persons. Vide Hales's Staticks, vol. 2.; and Ratty's experiments on Mrs Stephens's medicines.

S E C T. VIII.

Experiments with lime-water and several medicines.

46. **H**AVING dissolved *tartar solubilis*, *drach. i.* in lime-water, *unc. i. sem.* I put in it a piece of B, four grains, which, though kept in warm digestion five days and a half, had not lost any thing of its weight, but was become somewhat more friable.

47. IN a solution of a drachm of *nitre* in an ounce and a half of oister lime-water, I put a piece of the same *calculus* of four grains, which, in five days and twelve hours, had near one grain of its substance dissolved.

48. HAVING immersed a fragment of B, seven grains, in oister lime-water, *unc. iii. sal cathart. amar. drach. i.*; after near four days warm digestion, the *calculus* had scarcely lost any of its weight, but its external surface was softer, and somewhat rotten.

49. AT the same time I put a piece of B, six grains, in oister-lime, *unc. ii.* in which was dissolved *sal Glauber. scrup. ii.*; after being kept near four days in a moderate heat, its surface was rather more rotten than the *calculus* in the last experiment; but it had lost none of its weight.

50. A piece of B, six grains, by digesting warm in oister lime-water, *unc. iii. sea-salt, drach. i.* betwixt three and four days, had a grain of its weight dissolved.

LIME-WATER does not remarkably dissolve most of the above salts, for the greatest part of them, after standing a little, falls to the bottom; upon which account I kept the *calculus* in these experiments suspended in the middle of the phial by a thread.

HENCE we see, that salts, even those of the neutral kind, destroy considerably the virtue of lime-water.

51. I put a piece of B, four grains, in a solution of seven grains of *aloes*, in oister lime-water, *unc. ii.*; which, by thirty-six hours warm digestion, was reduced to about three grains.

52. I

52. I infused *pulv. rhei* gr. x. in oister lime-water, *unc.* iii. for twelve hours; after which I immersed in it a piece of B, six grains; in thirty-two hours warm digestion, near two grains of it were rotten and dissolved.

It is observable that lime-water, mixed with powder of rhubarb, immediately acquires a deep red colour, as if cochineal had been infused in it; and the same thing happens when an infusion of rhubarb is mixed with stale urine or potash; whence we may see why the urine of a person who has taken rhubarb acquires a bloody colour, if it remains but ever so short a time in a pot, which is crufted with sediment of stale urine. A *phenomenon* which has at first given uneasiness to not a few who were ignorant of its true cause.

53. HAVING infused in the same manner *pulv. jalap.* gr. x. in oister lime-water, *unc.* ii. I put in a fragment of B, six grains, which, by digesting warm thirty-two hours, was reduced to five grains.

54. A piece of B, four grains and a half, being for thirty-four hours digested warm, in an infusion of *fenna drach. sem.* in oister lime-water, *unc.* iii. lost one grain.

55. HAVING dissolved manna, scrup. ii. in oister lime-water, *unc.* ii. I immersed in it a piece of B, four grains and a half; which, by being kept thirty-four hours in a moderate heat, had above a grain of its substance rotten and dissolved.

FROM these experiments we see, that if, by drinking lime-water, the body should be rendered costive, which (especially if no soap is taken along with it) may be often the case, it will be better to use some of the last mentioned purgatives, than any of the salts in the beginning of this section.

S E C T. IX.

Experiments shewing the change made on lime water by boiling, and being exposed to the open air; with further observations on its nature and use in several diseases.

HAVING in the above sections made a variety of experiments with lime-water upon different substances, it seemed proper next to try what alteration would happen to it from boiling, or exposing it to the open air, and to inquire wherein its virtue consists.

56. TWELVE ounces of lime-water being boiled pretty quickly into four, had lost some of its virtue; for whereas, before boiling, a blackish colour was produced by two parts of it to one of claret, now it required near two and a half. Nay, Dr Langrish tells us, that upon distilling a pint of lime-water from two quarts, he found both the distilled water in the receiver, and what remained in the retort, impaired in their virtues *.

THE vapour that arises from hot water during its ebullition with quick-lime, is only an insipid and inodorous water, quite destitute of the virtues of lime †.

57. IF a bottle be filled with lime-water, and closely stopt, it will keep for a long time, without suffering the least change, or losing any of its virtues: but, having exposed four ounces of it in an open vessel, it began very soon to throw up a scum, and let fall some sediment of the same nature; in three days it had lost much of its taste, and ceased to turn claret blackish; and in five days, when the taste of the lime was almost quite gone, it neither changed the colour of syrup of violets, nor had any effect in dissolving the stone. And this happens equally soon, when placed in the cold air, as in a moderate heat: but depends a great deal upon the narrowness or wideness of the vessel: for it will be found, that the time in

* Physical Experiments on brutes, p. 13. 15. and 16.

† Langrish's Physical experiments on brutes, p. 13. and 14.

in which lime-water, thus exposed, loses its virtue, will be more or less, according to the proportion which the surface bears to the quantity of the fluid.

THE scum which lime-water, exposed to the air, throws up, is at first an extremely thin pellicle, exhibiting various colours like a rainbow, or soap-bubble; these colours, however, gradually change, till, by the constant apposition of new particles, the scum becomes thick enough to reflect all the rays of light equally, and so appears white.

THIS crust which lime-water affords, being well beat, and mixed with syrup of violets, and then some common water added to it, the mixture, after standing a little, acquires a green colour. The parts of this scum are so minutely divided, and intimately mixed with the water when it is first poured off the lime, as to be absolutely invisible, and to remain inseparable from it, as long as it is kept in a close vessel. Why they should immediately begin to separate from the water, and unite together when exposed to the open air, may not be easy to account for. But is not lime-water rendered weaker by boiling, because its parts are thus more exposed to the air, and not by the heat's expelling any thing out of it? And does not lime-water, when distilled in close vessels, lose some of its virtue, because its parts, when elevated in the form of a vapour, are more exposed to the action of the air in the retort and receiver? Is not this rendered probable by Dr Black's observation, *viz.* That though *magnesia alba* which abounds with air, destroys the virtues of quick-lime and its water, yet, after it is deprived of its latent air by calcination, it has no such effect *?

58. THE calcarious matter which separates from lime-water upon its being exposed to the air, can by no art be again dissolved in water, or intimately united with its particles, but perpetually falls to the bottom, in the form of a white powder, which is altogether insipid, and void of taste.

B b b

(I.) THIS

† *Dissertatio inauguralis, De magnesia alba*, p. 31. 32. 33. and 37.

(1.) THIS calcarious matter being mixed with vinegar, a strong ebullition ensues, which lasts for a considerable time.

(2.) I put a small quantity of this powder of lime-water in a silver-spoon, and kept it over a pretty brisk fire for fifteen minutes; but after it was taken from the fire, and cooled, it raised the same effervescence with vinegar as before.

(3.) I poured boiling water once and again upon some of this calcarious matter, to see if, by repeated affusions of the water, it would lose any thing; but, after the water was drained off, and the powder moderately dried, it effervesced with vinegar equally as before.

HENCE the ebullition which the calcarious matter of lime-water makes with acids, seems neither to be owing to a volatile nor fixed alkaline salt; for a volatile salt would have been expelled by the fire, and either a fixed or volatile one dissolved by repeated affusions of boiling water.

(4.) POWDERED chalk makes a considerable and lasting ebullition with vinegar, in the same manner as the scum of lime-water, and continues to do so after repeated affusions of boiling water, or being exposed to the heat of a brisk fire.

HENCE the calcarious matter afforded by lime-water seems to be a true *alkali terreux*, like chalk, and its effervescence with acids to be owing to this alone.

WHEN lime-water is mixed with vinegar in the proportion of ten to one, it does not throw up any crust or scum, because the calcarious matter which it contains is dissolved by the vinegar.

59. IT has generally been thought, that no salt could be got from lime-water: nor could I by evaporating it ever procure any; the scum left behind having rather the appearance of a fine lime, or absorbent earth. If it be said, the salt in lime-water is of the volatile kind, and therefore not to be got by evaporation, it may be sufficient to answer, that then this volatile salt, which should in some degree affect our smell, ought to be procured by distilling lime-water in close vessels: But it is so far from being true that there is any volatile salt in lime-water, that the vapour which rises from

from water during its ebullition with quick-lime, is intirely devoid of the virtues of lime, and scarcely differs from common water. Yet, upon adding one part white-wine vinegar to ten or twelve parts stone lime-water, after several days, I perceived some saline concretions adhering to the sides of the glass: these tasted not unlike sea-salt, but sweeter, and, no doubt, proceeded from the acid salt of the vinegar rendered neutral by the lime-water. Having infused a fragment of B in some oister lime-water, made with shells that had lain fifteen hours after being taken from the fire, I was surpris'd to observe, in three or four days, a prodigious number of small pointed chrystallisations, like fine needles, about the sixth part of an inch long, darting as it were into its surface, and giving it somewhat the appearance of a hedgehog. But I am apt to think, that these did not proceed from the lime-water, but the sea-salt, with which the oister-shells, even after calcination, so much abound *. And accordingly I have since often observed the same saline chrystallisations, tho' not so remarkable, produced by lime-water made with oister-shells newly got from the sea; although, as far as I remember, never from stone lime-water, nor that made with calcined shells, which, by being long expos'd to the weather, had been intirely deprived of their sea-salt. We are told indeed, that Mr Leeuwenhoek discovered, by his microscopes, in lime-water, a great number of saline rigid particles †. But whether his imagination assisted him herein, or whether he did not rather want to discover a similitude betwixt this water and that got off burnt *calculi*, and the tophaceous matter which sometimes issues from the joints of gouty persons; or whether there really are such saline particles, I will not take upon me to determine: For altho' it has been the general opinion of the most eminent chymists, that no salt could be procur'd from quick-lime; yet, of late, Mr Du Fay ‡ pretends to have extract'd a salt from lime-water, and has given an account at large of the way in which this may be done. But, since his salt is of the neutral kind, and does not seem to be possess'd of

any

B b b 2

* See No 67. below.

† Musgrave de arthritide, cap. 9. § 4.

‡ Memoires de l'Acad. des sciences 1724.

any remarkable virtues; and since he observes, that, unless the lime-stone is put red hot into the water, and boiled with it, and its water poured out while it is yet boiling in order for evaporation, it either affords no salt at all, or very little; we may safely conclude, that the virtue of lime-water does not lie in this salt.

60. ALTHO' lime-water changes the blue colour of syrup of violets into green, and affords an alkaline absorbent earth, which effervesces with, and destroys acids; yet as the water itself does not effervesce with vinegar or spirit of vitriol, it seems to partake but little of an alkaline nature. Nor do the virtues of quick-lime consist in an *alkali*: for quick-lime effervesces much less with vinegar than with small-beer, and is very difficultly flaked by either; while water, which is neither acid nor *alkali*, being poured upon it, produces great ebullition and heat, and quickly dissolves it. And although quick-lime makes a great ebullition with the stronger acids, as spirit of vitriol, nitre, and sea-salt; yet this arises from the terrestrial *alkali* which it contains in common with flaked lime, the scum of lime-water, and the other absorbents, and not from any particular salt of that nature. Nor is the activity and corrosive power of quick-lime owing to its alkaline nature; since the calcareous matter of lime-water, which is insipid, and altogether void of the peculiar taste of the lime, makes a greater effervescence with vinegar than it, and since Mr Homberg has observed that flaked lime requires as much spirit of nitre, or sea-salt, to saturate it as quick-lime*.

THE drinking of lime-water does not render the urine alkaline: for Mr Millar's urine neither effervesced with vinegar, nor turned syrup of violets green; altho' he alledged he could perceive the taste of the lime-water in it.

HENCE we may conclude, that the strong-scented alkaline urine voided by such persons as have taken Mrs Stephens's medicines, was not so much owing to the lime in them, as to the alkaline salt or potash, which makes up so considerable a part of the soap. And the

* Memoires de l'Acad. des sciences an. 1700.

the dissolving virtue of such urine does not seem to consist (as Dr Kirkpatrick and the French academists think *) in its alkaline nature; since we find in Mr Millar's urine this power without that quality; and since Dr Hales has shown, that the potash, which is almost the only alkaline ingredient in these medicines, has little effect in dissolving the stone †.

ALTHO' quick-lime appears, by its effects, to have a very considerable affinity with fixed alkaline salts, yet in many respects it differs from them; and there are not wanting experiments which seem to indicate an acid quality in it. Hence several chymical writers have been of opinion, that quick-lime contained both an acid and a fixed alkaline salt, and to the conflict between these opposite salts they have ascribed the ebullition and heat which is produced when quick-lime is dissolved by pouring water upon it ‡. Nor is it any wonder, say they, that, by the affusion of water, no salt is got from quick-lime, because its two opposite salts are, by acting on each other, destroyed, and turned into a third substance, which, like all magisteries, is insipid, and not dissolvable in water.

LIME-WATER being mixed with salt of tartar, immediately becomes turbid and whitish, and after some time lets fall a white insipid powder, which greatly resembles prepared oyster or egg shells, and the crusts of lime-water; for it is an absorbent alkaline earth which effervesces strongly with vinegar and spirit of vitriol, but is not dissolved by the latter. It has been said that this earthy powder which is precipitated by mixing salt of tartar with lime-water, proceeds chiefly from the salt §. But the following observations will shew that it comes chiefly, if not wholly, from the lime-water.

1. THE calcarious matter precipitated by mixing salt of tartar and lime-water, is in proportion to the quantity and strength of the lime-water, but not of the salt.

2. SOAP-LEYS mixed with lime-water, produce the same precipitation as salt of tartar, or a solution of it in common water: but it is

* Kirkpatrick's case; and Memoires de l'Acad. des sciences an. 1739 et 1740.

† See below, No 62.

‡ Mayow oper. cap. 14. de æstu calcis vivæ.

§ Idem ibid. § Allston's first Dissertation on quick-lime, edit. 2. p. 64.

is by no means probable, that the salt in the soap-leys, which has before sustained the action of quick-lime without being converted into an earthy powder, will suffer this change by the much weaker action of lime-water.

3. If the white powder precipitated upon mixing salt of tartar with lime-water, proceeded from the salt, a solution of this salt in water should be rendered turbid and milky, and produce a greater precipitation, by having a piece of calcined oyster-shell immersed in it, than by being mixed with lime-water; which however is not the case. For, having dissolved a dram and a half of salt of tartar in something more than two ounces of water, and filtered the solution through grey paper, I immersed in it three or four pieces of calcined oyster-shells, weighing about two drams; a hissing noise immediately ensued, and a good many air-bubbles arose, but the liquor remained equally pellucid as before; nor was there any turbid milky appearance in this ley, during four days that it stood on the lime, and scarce any observable precipitation of the salt; I say observable, because it was not easy to distinguish a small precipitation from the lime-powder, a little of which fell from the shells to the bottom of the glass.

4. WHEN two or three drops of lime-water fall into a solution of potash or salt of tartar, they become immediately white; but this appearance soon after vanishes, because so small a quantity of lime-water affords too little earthy matter to be visible when dispersed through the whole solution of the salt. If an ounce of lime-water mixed with salt of tartar gives only a grain of calcarious matter, one drop will not give $\frac{1}{500}$ part of a grain, which is too small to be visible, when it falls to the bottom of the glass in the form of a powder. But on the other hand, a few drops of a solution of salt of tartar or potash, let fall into a glass full of lime-water, render the whole turbid and milky, and precipitate a white powder; because a very little of the solution is sufficient to separate a considerable quantity of calcarious matter from the water.

WHEN

WHEN lime-water destroys acids, we readily ascribe this effect to the alkaline earth which it contains: But to what principle in lime or lime-water are we to refer the precipitation which happens upon the mixture of lime-water with a fixed alkaline salt? Does it shew that there is an acid in lime-water? that this acid is united closely with an alkaline earth, which it keeps invisibly suspended in the water, but that being strongly attracted by the fixed alkaline salt, it leaves this earth, which then precipitates in the form of a white powder? Are we to ascribe it to this acid, that spirit of *sal ammon.* distilled with quick-lime, has its nature so changed as not to effervesce with acids: Does it seem probable, that certain stones and animal-shells are, by calcination, changed in quick-lime, because, as alkaline earths, they are peculiarly qualified to receive and unite with this acid? And may we conclude from the above experiment, that the active properties of quick-lime are owing to an alkaline earth heightened, perhaps, by the fire, and united with a subtile acid spirit? Whoever should answer these queries in the affirmative, would be greatly too hasty in his conclusions; for the following experiment will shew, that though fixed alkaline salts render lime-water turbid, and cause a precipitation, yet their alkaline quality is not destroyed by it.

HAVING added forty grains of salt of tartar to twenty ounces of strong stone lime-water, after some hours, I poured off the clear water; and boiled it into three ounces and a half; when it effervesced strongly both with vinegar and spirit of vitriol. Further, if there was any acid in quick-lime, would a solution of potash have its pungent corrosive qualities greatly heightened by being poured upon it? Ought not rather its alkaline nature to be thence greatly impaired or destroyed?

MR GEOFFREY thinks, that there is in quick-lime a fixed alkaline salt, formed of the aluminous, vitriolic, or nitrous acid of the stone, and of the acid in the wood or coals*. This salt he imagines to be, like the fixed *alkali*, united with flint and sand in making glass, so intimately conjoined with the earthy parts of the
lime

* Memoires de l'acad. des sciences 1720. edit. 8vo. p. 28.

lime as not to be separable from them by any affusion of water. But if this be so, it must be allowed that lime-water cannot owe its virtues to this salt; nor by consequence lime itself, whose virtues are of the same kind, though vastly stronger. If it be said, that lime-water contains some of the finer parts of the lime, to which its virtues are owing; we answer, that as the calcarious matter which lime-water affords, is a mere alkaline earth, the supposed salt must have left it, and consequently cannot be so inseparably united with the earthy parts of the lime as is supposed. Further, as quick-lime, after being rendered insipid by frequent affusions of water, acquires its former properties by a new calcination*, it evidently follows, that no peculiar salts are required in animal shells, stones, or chalk, in order to their being changed into quick-lime by calcination.

NAY, if lime-stone be, before calcination, impregnated with alkaline, acid, or neutral salts, it will not, by being burnt in the most intense fire, acquire the properties of quick-lime†. And hence we may see, why quick-lime, whose virtues have been extracted by a ley of an alkaline salt, does not become quick-lime again by a new calcination.

SINCE then there does not appear any good reason to ascribe the virtues of quick-lime or lime-water to any acid or alkaline salts; it may be asked, what is the nature of quick-lime, and whence do its active qualities proceed? To which I answer, that it is an alkaline earth, which acquires, by calcination, highly acrid, penetrating and igneous properties; and that, as the native salts of vegetables are, by the action of the fire, converted into a fixed alkaline nature, so the earthy matter of animal shells, and certain stones, is, by burning, changed into an acrid caustic substance, which, however it may agree in some things with these salts, differs from them in many respects, and is of a nature peculiar to itself.

THE

* *Memoires de l'acad des sciences* 1700, edit. 8vo. p. 160.; et *Macquer Elemens de chimie theorique*, p. 66.

† *Macquer Elemens de chimie theorique*, p. 68. &c.

THE learned Dr Joseph Black professor of medicine and chymistry in the university of Glasgow, has, since the second edition of this Essay was published, given an account of the nature of quick lime, which is equally new and satisfactory *.

FROM his curious experiments and observations, it appears, that lime-stone or shells, when they are converted by the fire into quick-lime, suffer no other change than the loss of their fixed air, and a small quantity of water. That the acrimony and corrosive quality of quick-lime are owing solely to its being deprived of its air. That as soon as it regains its air, it is reduced to a calcarious earth, and is indissolvable in water. That soap-leys are much more acrid and caustic than a *lixivium* of potash, because this salt in the soap-leys is deprived of its air, which is more strongly attracted by the quick-lime.

FROM this account of the nature of quick-lime we may see why lime-water soon throws up an earthy crust, and loses its virtues by being exposed to the air: Why that portion of quick-lime which is dissolved in lime-water, and therefore invisible, is immediately converted into a calcarious earth, by mixing salt of tartar or potash with lime-water: Why quick-lime is much more weakened by being long exposed to the open air, than by being kept under water: Why the *lapis infernalis* or dry salt of soap-leys is rendered much less acrid, and loses its power of dissolving the stone by repeated solutions and evaporations †. Why the *lapis infernalis*, which is a fixed alkaline salt deprived of its air, is much more caustic than quick-lime, which is only a calcarious earth void of air: Why the spirit of sal ammoniac prepared with quick-lime, does not effervesce or emit air, when mixed with acids; and why the crusts of lime-water or quick-lime, after being fully saturated with air, and deprived of its virtues, may, by a new calcination, be again converted into quick-lime.

IF the caustic salt in soap-leys differs only from potash in being deprived of its air, which is attracted by the quick-lime, it must

C c c

follow,

* Edinburgh Physical Essays, vol. 2. art. 3.

† Vid. Hales's Exp. on Mrs Stephens's med. No. 20. & 21.

follow, that there is no lime or calcarious matter either in soap-leys or that salt. Dr Hales, however, having dissolved 320 grains of the *lapis infernalis* or dry salt of soap leys, in hot water, and filtered the *lixivium* through paper, found 11 grains of earthy matter, *i. e.* $\frac{1}{29}$ part of the whole *; which is about four times as much as 320 grains of pure potash would have afforded, if it had been treated in the same manner. But it is not improbable, that even this small quantity of earth proceeded, either from the impurity of the ley used by Dr Hales, from the vessel in which it was evaporated, or from the alkaline salt †, which, by the action of the quick-lime upon it, may perhaps be rendered more easily convertible into earth than it was in its natural state. And is not this last supposition confirmed by those experiments which shew, that when equal parts of quick-lime and potash are infused in water, $\frac{1}{3}$ part more of the latter is converted into earth, than would have been by dissolving it in water alone ‡?

61. FROM

* Experiments on Mrs Stephens's medicines, p. 8.

† I evaporated away in a white stone-bason, over a very slow fire, about four ounces of a ley of purified potash and quick-lime, which I had kept in a corked bottle two years and a half; and obtained from it 234 grains of a dry white salt, which being carefully dissolved in six ounces of cold water, and filtered through grey paper, yielded no more earthy matter, than the same quantity of a pure alkaline salt would have done, if treated in the same manner. Not satisfied with this, I poured 14 ounces of water upon three ounces of common potash, and five ounces of oyster shell-lime that had been infused in water for 14 days. Having frequently stirred this mixture, I decanted off some of the clear ley after it had stood ten hours on the lime, and having evaporated it as above, I dissolved 124 grains of the dry salt in boiling water, and filtered it; the matter which remained in the filtering paper feeling very smooth and oily, I mixed it well with some cold water, and filtered it again, when there scarce remained $1\frac{1}{2}$ grain of earthy matter, *i. e.* very little more than a pure alkaline salt would have afforded.

At another time I evaporated away, in a brown glazed earthen vessel, some of the first mentioned ley of potash and oyster-shell lime, and obtained 174 grains of a dark brown coloured salt, which being dissolved in cold water and filtered, left in the filtering paper six grains of a brown earthy sediment.

These experiments seem to shew (contrary to what some have thought (a)) that pure soap-leys contain very little, or rather none of the earthy part of the lime; and, that when they have afforded a greater quantity of calcarious matter, it may perhaps have been owing to the vessel in which they were evaporated.

‡ Alston's 1st dissertation on quick-lime, p. 17.

(a) Dr Alston's Dissertation, edit. 2d. p. 19. 72.

61. FROM the great affinity which has been generally supposed by medical writers betwixt the gout and gravel, it might perhaps be worth while to try the effects of lime-water in this disease also. And the learned Dr Cheyne having asserted, that the gouty chalk-stones were, as to their essential qualities, the same with gravel stones, and that they yielded both the same principles, when chymically treated *, I procured some of the former, and infused them in lime-water. At first, being specifically lighter than the water, they swimm-
 ed. But, after emitting a great number of air-bubbles, they soon fell to the bottom, and, in a day or two, were become as soft as butter. But, having afterwards infused a piece of this gouty matter in common water, I found precisely the same effects from it as from the lime-water: so that whatever probability there may be of lime-water doing service to gouty people, from the supposed affinity betwixt it and the gravel, yet nothing can be drawn from this experiment. However, lime-water promises to do as much, as an alterative, in several chronical diseases as many other medicines. It may be taken in large quantities, and be long persisted in. Its parts are so subtile, that they can penetrate, at least, where-ever water can go †; and consequently must pass thro' the smallest vessels in the human body. When mixed with the blood or urine, it seems to exalt their salts and oils, and upon the solids it acts partly as an astringent. Hence it ought to be of use where the blood is watry, sluggish, viscid, and unactive, and the solids weak or relaxed. In the *fluor albus* and *diabetes* its virtues are conspicuous; and it has been of good use in the *scrophula*. In *diarrhæas* and dysenteries, acidities in the *primæ viæ*, and in excoriations or ulcerations of the guts, it is an excellent remedy. From its penetrating, dissolving ‡, and detergent qualities, there is reason to expect benefit from its use in

C c c 2

chronic

* Cheyne on the gout, p. 72. edit. 4.

† See No. 57. above.

‡ It has been observed, that when *ichthyocla* is dissolved by boiling it in lime-water, it loses its glutinous quality; and fizy blood let from a vein into a cup half full of tepid lime-water, has its crust somewhat thinner and less tough, than when it is mixed in the same manner with common water. I have also observed, that the tough phlegm with which the stomach is almost always loaded in some patients, is dissolved by lime-water.

chronic rheumatisms, and *sciatica*, and other obstructions in the smaller vessels. I have been told of its having had good effects in low nervous fevers: But in ardent and putrid fevers it ought to be hurtful, on account of its power of volatilising the salts, and corrupting the oils of the animal humours*. In those cutaneous eruptions, which are commonly but improperly termed scorbutic, it sometimes proves a sovereign remedy, at other times fails. I once cured a wet scorbutic eruption in the hands, after a mercurial salivation had been found ineffectual, by causing the patient drink above an English pint of oyster-shell lime-water every day for two months, and now and then wash her hands with it.

S E C T. X.

Experiments with lime-water, soap-leys, soap, &c.

AS it appears from Dr Hales's experiments, that soap is possessed of a considerable power of dissolving the stone, and that this is chiefly owing to the lime that enters into its composition, I was excited to make some experiments upon it, with a view still further to discover wherein its virtue lay, and what proportion it bears to that of lime-water; whether lime-water might not be improved by it; and how far their lithontriptic quality is destroyed by the same things.

62. HAVING dissolved potash, *drach. ii. sem.* in boiling water, *unc. iv.* I put in it a piece of A, weighing nine grains, which, in fifteen days warm digestion, lost very little of its weight, nor was its surface softened to any depth. However, its substance seemed to have been rendered more friable; for, upon pouring boiling water on it, it rent in several places.

63. I boiled stone lime-water, *unc. xiv.* with potash, *drach. v.* into *unc. vi.* and having laid a piece of A, of eleven grains and a half, in

* See above, No. 10.

in it, found that, after standing twelve days in a gentle heat, it had lost seven grains and a half of its weight.

64. A pound and a half of boiling water, in which an ounce of potash was dissolved, being poured on two ounces and a half of quick-lime, afforded, after the ebullition was over, and the lime had fallen to the bottom, a very fiery corrosive liquor; which, if applied to the tongue, in the smallest quantity, was in hazard of bringing off the skin. A piece of A, ten grains and a half, was dissolved in fifteen or sixteen hours, in a moderate heat. A *lixivium* of the same kind, which I made afterwards, but which did not seem so strong, dissolved a piece of B, three grains, in twelve hours; while a fragment of the same *calculus*, of fourteen grains, required three days and six hours of a cold maceration in *aqua fortis simplex* * to dissolve it.

THE great dissolving power of this *lixivium* must be ascribed to the change made on the alkaline salt by the quick-lime, (See above, p. 85. and 86.), for a ley of potash has little or no effect on the stone.

INSTEAD of an ounce, I dissolved *drach. 2.* of potash in *lib. 1. sem.* of water, and poured it on quick-lime as above, thinking that the *lixivium* thus procured would be possessed of a greater power of dissolving the stone than simple lime-water, and at the same time not so acrid as to be in danger of destroying any parts of the human body; but I soon found that any advantage I expected from the greater lithontriptic virtue of this *lixivium*, was more than balanced by the acrimony and nauseousness communicated to it by the alkaline salt.

64. HAVING found a much greater power of dissolving the *calculus* in shell than in stone lime-water †, I thought it worth while to try whether a ley made with shell-lime and potash would prove a stronger lithontriptic than common soap-leys. For this purpose I poured two pounds of boiling water upon four ounces of purified potash, and five ounces of calcined oyster-shells fresh from the fire, and

* Pharmacop. Edinburgens.

† See above, No. 14. 18.

and let them stand twenty-four hours. Then into a phial-glass filled with this ley, I put a piece of B, thirteen grains, which, after ten hours warm infusion, was intirely dissolved. At the same time I put another piece of B, of the same weight, into the common ley with which they make soap at Glasgow. After sixteen hours warm infusion, it was mostly all dissolved into a white powder; and the very small *nucleus* that remained was quite rotten, and when pressed between one's fingers, fell down into a white mucilage.

THIS ley, made with oyster-shell lime, was as pellucid as water, and not so disagreeable to the taste as the Glasgow soap-leys; for, when mixed with twelve times its quantity of water, it was less nauseous, and seemed not much more acrid than the other diluted with sixteen waters. Its specific gravity was about one twenty-fourth part less than that of the common ley. Notwithstanding all which, we see its dissolving power was above one third stronger.

HENCE, in cases where it is thought proper to order soap-leys for the cure of the stone, it plainly appears, that a ley of potash and shell-lime is preferable to common soap-leys; since it is less disagreeable to the taste, and possesses a much greater dissolving power.

WHILE the Glasgow soap-leys, lime-water, and a solution of soap, corrode and break down the *calculus* into a white powder, or rotten scales, and so are, properly speaking, only lithontriptics, the ley of potash and oyster shell-lime melts down the stone into an unctuous substance, which is mostly all suspended in the pores of the *menstruum*; and therefore, like *aqua fortis*, or spirit of nitre, may be properly called a solvent of the stone.

WHEN a fragment of B was immersed in this ley it immediately sent forth from every part of its surface small streams, as it were of an oily fluid, exhibiting the same appearance in the ley as rum does when mixed with water. This may be observed by holding the phial in which the *calculus* and ley are contained between one and the light. And though it is most remarkable after the first immersion, yet it continues, in some degree, for a considerable time.

THE *calculus* immersed in the Glasgow ley, did not exhibit any such appearance; but its surface quickly became all over white.

To

To two ounces of purified potash, and three and a half ounces of oyster-shell lime, I added twelve ounces of boiling water. After twenty-four hours, I decanted off the ley, and poured it upon fresh calcined shells; by which means it acquired a yet stronger virtue of dissolving the *calculus*: for a piece of B, thirteen grains, was, after eight hours warm infusion in it, totally dissolved. Although this ley dissolved the stone twice as fast as the Glasgow soap-leys; yet, mixed with equal quantities of water, it was less nauseous to the taste, tho' somewhat more pungent.

FROM these experiments, it is reasonable to think, that if the *sapo amygdalinus* of the London Dispensatory were made with a ley of purified potash and oyster-shell lime, instead of common soap-leys, it would be full as agreeable to the taste, and possessed of a greater power of dissolving the stone.

65. A piece of A, of seventeen grains and a half, being put in a solution of Alicant soap in warm water, had, after six days warm digestion, a pretty thick white crust all round it ready to fall off; which being removed, the undissolved part weighed fourteen grains. In nineteen days it was reduced to six grains.

IN order to know whether the virtue of lime-water might not be increased by its mixture with the soap, I put a piece of A, eighteen grains, in a solution of soap in stone lime-water, which, after lying five days in it, in a moderate heat, was reduced to six grains, several white crusts having fallen off in this time. But finding it had lost none of its weight, and that no farther sensible impression was made on its surface, after standing twenty-four hours longer, and observing that the bottle was not very closely corked, I suspected that the solution had lost its virtues, which evidently appeared by its having lost all taste of the lime-water; I therefore put the *calculus* in a new solution, where in three days it was all dissolved, except a small *nucleus* weighing a grain.

HENCE it appears, that a solution of soap in stone lime-water, has a considerably greater dissolving power than either a solution of soap in common water, or than stone lime-water itself.

[66.] ANOTHER time, when I made the same experiment, I did not find the virtue of the lime-water so much increased by the soap: For a piece of B, ten grains, put into the like solution, in two days and nine hours, only lost a little more than three grains; while a piece of the same weight, in stone lime-water alone, lost in that time two grains. Whether the soap in the one case was better than in the other, or whether the due proportion in mixing them was not hit in this last experiment, I cannot positively affirm.

It may be proper to observe, that the lime-water must be pretty hot; and the soap and it must be agitated together for a considerable time, otherwise they will not unite.

THE quick dissolution of the stone in the two last experiments, affords a clear reason for the success which attended the stone lime-water in Mr Millar's case; which we ought to consider as having had its efficacy in a good measure heightened by its being taken along with a considerable quantity of soap.

67. HAVING found the dissolving virtue of oyster-shell lime-water much greater than that of stone-lime, I thought a solution of soap in it would have proportionably greater effect. But here I was disappointed: For I found it impossible by any art to make them unite; which seems to be owing to the sea-salt that abounds in the oyster-shells, and which the calcination does not entirely destroy. However, as the cockle-shell lime-water (N^o 17.) dissolved soap, I suspected, that, if oyster-shells were long exposed to the weather before calcining, the water procured from them might probably do so likewise. Accordingly, having got some that had lain long upon the sea-shore, and burnt them, I found the lime-water they afforded mix with soap as well as any other*: But neither it, nor the cockle-shell lime-water, seem to have their virtue sensibly increased by the soap.

[67.] HAVING

* I boiled some oyster-shells four or five hours, changing the water thrice in that time, thinking that this might free them of their salt, and render their lime-water miscible with soap; but without any effect. However, I would advise the boiling and washing of these shells before they are calcined; as it seems to free the lime-water of somewhat of a fishy taste it otherwise has.

[67.] HAVING poured a very weak solution of soap upon some calcined oyster-shells, I procured a liquor tasting somewhat of soap, and strongly of lime; which, in thirty-eight hours warm digestion, reduced a piece of B, four grains, to one grain and one third of a grain.

AFTER this I dissolved three drams of soap in thirty-five ounces of boiling water, and poured it on five ounces of calcined oyster-shells. The *lixivium* which they afforded, tasted strong of the soap as well as lime, and was pretty pungent and disagreeable. Being mixed with urine, it produced the same appearance as simple lime-water *; but raised a smell something like that of burnt horn. A piece of B, three grains and a half, being immersed in it, was, in twenty-four hours warm digestion, reduced to one grain.

68. A solution of soap in fresh small-beer, had no effect in dissolving some pieces of A, although allowed to lie eight days in it, in a moderate heat.

69. A solution of soap made in one part of malt spirits, and two parts of water, has very little virtue, though more than the preceding.

THUS we see, that the dissolving power of soap, as well as lime-water † is destroyed by fermented liquors, and greatly weakened by spirits; and consequently, that it is proper for such as use soap and lime-water to abstain from those liquors.

70. I put a piece of B, seven grains, in a solution of Alicant soap; which, by being kept in a gentle heat for four days, lost two grains of its weight.

71. AT the same time, I infused two pieces of B, each weighing eight grains; the one in two ounces of the above solution, with a dram of white sugar; and the other in two ounces of the same, with a dram of honey: The *calculus* in the solution with sugar lost about two grains in four days warm digestion, while the other was scarcely diminished in weight above grain.

As in refining sugar a good deal of lime-water is employed, this may probably be the reason why it destroys the virtue of lime-water

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ter

* See No. 8. above.

† See No. 24. 25. 26. and 30. above.

ter less than almost any other substance, and seems scarce to weaken the dissolving power of soap at all.

FROM this experiment it appears, how much the lithontriptic virtue of Mrs Stephens's medicines must be weakened, by her ordering the decoction to be sweetened, and the pills to be made up with honey; and that in place of it the *syrupus de saccharo* and sugar may be substituted with considerable advantage.

72. I infused a fragment of B, five grains and a half, in a solution of Alicant soap, made in a strong decoction of asparagus; which, after digesting warm for five days, had lost near two grains.

73. MOST people have, doubtless, had occasion to observe, that while the internal part of Alicant soap is of a blue colour marbled with white, its surface, which is exposed to the air, is reddish, and sometimes yellow or white. Now, as by the above experiments we have found, that the lithontriptic virtues of soap and lime-water are in many instances destroyed by the very same things; and as lime-water, by being exposed to the air, is soon rendered effete, I made the following experiment, to see, if that part of the soap, which has its colour changed by the air, possesses less virtues than the rest.

HAVING dissolved some of the internal blueish part of Alicant soap in warm water, I infused in it a piece of B, six grains; which, in three days warm digestion, lost near two grains.

AT the same time, I made a solution of equal strength, of the external part of the same soap, in warm water, and immersed in it a fragment of B, six grains: after digesting fifty-eight hours warm, and fifteen cold, it had only lost about three-fourths of a grain.

“FROM this experiment it is plain, that such as swallow soap for the stone or gravel, should carefully throw aside that part of it which has had its colour changed by the air; and as in very old soap I have seen this about one third of an inch thick, it is very probable, that two ounces taken with this caution will have as much effect as two and a half, when good and bad are swallowed both together.

^b HENCE it also appears, how improper it is to make soap into pills, unless they are presently to be used; and consequently how much

much Mrs Stephens's medicines must have suffered in this way. For, as the air, in rendering the soap effete, acts only upon its external surface, the more the surface is increased, the greater will the quantity be that is deprived of its virtue. Thus, suppose a cubical piece of soap of four inches, made into twelve or fifteen hundred pills, its surface, which was before only ninety-six, will now, perhaps, be near a thousand square inches; and consequently, in a given time, the pills must lose ten times more of their virtue, than such a piece of soap, if allowed to remain whole. Soap seems also to be rendered a good deal the worse when reduced to a powder; whereby, not only its lithontriptic power is weakened, by its surface being thus increased, and exposed to the air, but the watery and oily parts of the soap being mostly evaporated, leave the alkaline salt deprived in a great measure of that which was intended to correct it.

75. DR HALES having been lately informed, that oyster-shell lime-water, mixed with *spir. nitri dulcis*, in the proportion of an English pint of the former to half an ounce of the latter, was a more powerful solvent of the stone out of the body than the lime-water alone; in order to know the truth in this matter, he added half an ounce of dulcified spirit of nitre to a pint of oyster-shell lime-water, made by pouring a gallon of water on a pound of calcined shells, and having filled a phial of two ounces with this mixture, he put into it a piece of a large *calculus* X, weighing twelve grains. At the same time he put into a like phial, filled with the lime-water unmixed, another piece of the same *calculus* Z, weighing eleven grains. Both these phials were placed in a heap of dung, whose warmth was ninety-four degrees, according to Fahrenheit's thermometer.

AFTER forty-three hours, the surfaces of both these stones were covered with a white mucilage; but there was much less of this on the *calculus* X than on Z: the same difference was observed after sixty-three hours; but after this it became less sensible. In a few days after the phials were taken out of the dung, the lime-water unmixed lost its dissolving power entirely; but that to which the dul-

cified spirit of nitre was added, continued for two months, to turn the surface of its *calculus* to a very thin coat of white mucilage.

FROM this experiment, which the Doctor was so good as to communicate to me, it appears, that dulcified spirit of nitre rather weakens than increases the dissolving power of oyster-shell lime-water; but that this lime-water mixed with it retains a lithontriptic virtue much longer than it would otherwise do. Whether this dissolving power, which continues so long, be owing to the lime-water being preserved from becoming effete, by the *spir. nitr. dulc.*; or, whether it is not rather to be ascribed wholly to this spirit, which, when mixed with common-water, dissolves the stone*, I shall not presume to determine; though the latter opinion seems most probable.

HOWEVER, since dulcified spirit of nitre does not much abate the virtue of lime-water, and is itself possessed of a lithontriptic power, it may be safely given to patients who are under a course of lime-water for the stone: and, as it is a good remedy for wind in the stomach and guts, provokes urine, allays heat and thirst, prevents putrefaction, and restores a depraved appetite from corrupted humours, many cases may occur where it may be prescribed, to great advantage, along with the lime-water.

WE have observed above, (N^o 10.) that lime and its water volatilize the salts, and corrupt the oils of the animal humours; when therefore we meet with calculous patients, whose fluids have a more than ordinary tendency to putrefaction, it might be very proper to make them add a few drops of dulcified spirit of nitre to every draught of the lime-water.

76. A piece of B, twelve grains, being infused in spirit of sea-salt for near seven days, during the greatest part of which it was kept in a moderate heat, had only three grains of its substance dissolved.

By comparing this experiment with those of N^o 17. 18. 19. and [64.], it appears, that both soap-leys and oyster-shell lime-water are stronger dissolvents of the *calculus* than spirit of sea-salt.

If

* Rutton's Exper. on Mrs Stephens's medicines, sect. 4. cap. 35. and 36.

IF an ounce of spirit of sea-salt be mixed with eight or ten ounces* of spring-water, or lime-water, and poured upon calcined oyster-shells fresh from the fire, a great ebullition and heat ensue. After this is over, and the lime quite fallen to the bottom, a clear liquor remains above; which being filtered through a piece of flannel, is as pellucid, and void of colour as water. This ley has no smell; but a pretty strong saline, and somewhat pungent taste, with a small degree of astringency. If it retains any thing of the peculiar smell or taste of the spirit of sea-salt, this shews that the spirit has not been thoroughly saturated by the lime. To prevent this, I found it of use, to mix with the spirit of sea-salt and water, before I poured them on the lime, a small proportion of purified potash.

THIS ley of sea-salt and shell-lime has very little virtue in dissolving the *calculus*. It is true, that, after lying some time in it, in a moderate heat, the surface of the stone becomes white, and throws off a few rotten scales; but it is three or four times longer in dissolving than in oyster lime-water. And I have observed, that when no potash was added to the spirit of sea-salt, although the ley were pretty free of the peculiar taste of the spirit; yet it seemed to have no power almost at all of dissolving the stone.

HENCE we see, that while in soap-leys the dissolving power of quick lime is greatly heightened by the addition of an alkaline salt, which of itself has little or no lithontriptic virtue; this same quality of the lime is greatly weakened, or entirely destroyed, by an acid salt, which is naturally a solvent of the *calculus*.

THE quack-medicine sold at London, under the name of the *liquid shell*, said to be calcined shells, reduced to a liquid form, and discovered by Baron Shawenberg, a German nobleman, seems to agree in every quality with a ley of spirit of sea-salt, potash, and shell-lime †. They have precisely the same colour and taste; mixed with spirit of vinegar or spirit of vitriol, neither of them effervesce:

* According to the strength of the spirit.

† Dr Linden, in an appendix to his late book on mineral waters, has given us several processes for making the liquid shell; concerning the virtues of which, he talks more in the style of an empirick, than of a physician. His second method of making it, is by pouring a pound and a half of water on calcined oyster-shells and *sal ammon crud.* each a pound.

fervesce; Hence they they are not alkaline. With a ley of potash, they make no ebullition, but are turned into a white *coagulum*. Upon adding oil of vitriol to them, a violent ebullition ensues, with a strong smell of spirit of sea-salt, and a white *coagulum* falls to the bottom. When mixed with a solution of mercury in *aqua fortis*, they immediately precipitate the mercury.

A piece of B, four grains, after twenty-eight hours warm, and as long cold digestion, in a small phial full of the liquid shell, had only half a grain of its substance dissolved.

HENCE it appears, that this medicine is neither acid nor alkaline, contains little of the virtue of the calcined shells, and has but a very inconsiderable power of dissolving the stone. How justly then it is said to dissolve the *calculus* out of the body in a few hours, in a moderate heat, or has been extolled as a grand *alkali*, and a powerful solvent of the stone in the bladder, is left to every one to judge.

S E C T. XI.

Of the action of lime-water in dissolving the stone.

ALTHO' it is of much greater importance to mankind to know that a certain remedy is able to cure this or the other disease, than to be informed of the precise manner in which it produces this effect; yet, to investigate the operation of medicines in the cure of diseases, is not only a subject worthy of a physician, and highly entertaining to a philosophical mind, but is also of very considerable use in practice; for it is likely, that a medicine, whose real nature and manner of acting upon the human body are known, will be more judiciously and happily applied in the cure of diseases, than one, whose unknown nature and specific operation, scarcely afford any indication in what particular stages of a disease, or circumstances of the patient, it may be used with the greatest prospect of success, and the smallest chance of mischief; when it may be most

most proper to administer it, and when to refrain from its use. As therefore we have made it appear, that lime-water not only dissolves the *calculus* out of the body, but sometimes even while in the bladder; it will not, I hope, be looked upon as an useless inquiry, to endeavour to point out the particular manner of its operation.

THE *calculus* consists of water, earth, air, salt, and oil. Whatever *menstruum*, therefore, can separate any of these principles from the rest, will more or less dissolve or break the stone.

As lime-water does not unite with oil, it is not probable, that it dissolves the *calculus*, by acting on its oleaginous particles. The water which is in the stone, is not to be extracted but by the force of fire in calcination, and the earth is the most fixed and immutable of all its principles; the action of lime-water therefore in dissolving the stone must be either upon its salt or air. With respect to the first of these,

EVERY one knows, that quick-lime volatilizes crude *sal ammoniac*. and lime-water produces the same effect, though in a less remarkable degree. And, as the animal salts of the urine and *calculus* greatly resemble *sal ammoniac*. so we find that lime acts upon them all in the same manner. Thus urine mixed with quick-lime, sends forth a penetrating saline urinous vapour, which is stronger or weaker as the urine is stale or fresh, or as it is more or less impregnated with salts. Lime-water has a similar, but much weaker effect. *Vid.* Sect. 1. N^o 7. and Sect. 2. N^o 10. above.

A piece of a *calculus* which had been in my custody seven or eight years, being reduced to a powder, and mixed with quick-lime, upon adding warm water, and stirring the mixture, sent forth a weak urinous smell; but if this stone had been recently extracted from the bladder, it would have probably afforded a stronger and more penetrating vapour.

MAY 12. 1750. I put an ounce of gravel-stones, with a pound and a half of oyster-shell lime-water, into a well corked bottle, and, having observed that the stones seemed to dissolve no more after the first eight or ten days, (though kept in a heat of about 100 degrees of Farenheit's thermometer), on the 28th I poured off the lime-water,

water, and found about three drams of the *calculi* undissolved; the greatest part being mouldered down into a white chalk-like powder.

THE lime-water had a yellowish colour, and a very particular kind of urinous, stinking, sulphureous smell, not easily to be described in words. It had lost all taste of the lime, and, in place of it, had got a very disagreeable one, of the same nature with its smell. This putrid water, after being exposed to the air three days in an open vessel, lost entirely its disagreeable taste and smell, but retained its yellowish colour. Whence we are led to conclude, that as the colour of this water proceeded from the grosser oil of the *calculus*, which is not volatile, so its smell was owing partly to the more attenuated oil, and chiefly to the salt of the *calculus* volatilized, and changed into a penetrating vapour by the action of the lime-water.

IT is observable, that as *sal ammon.* mixed with lime-water, hinders it from throwing up any scum, so the lime-water in this experiment neither afforded any earthy crust while kept in the bottle, nor afterwards when exposed to the open air.

NOR does lime act thus in volatilizing the salts of the urine and *calculus* alone; for blood fresh drawn from a vein, being mixed with equal parts of lime-water, immediately acquires a kind of burnt urinous smell.

FROM what has been hitherto offered, it may seem to follow, that lime-water dissolves the stone, chiefly by volatilizing its salt: But then, as fixed alkaline salts volatilize *sal ammon.* and the salt of urine as well as quick-lime, it might be expected that a solution of potash, or salt of tartar in water, should also dissolve the stone; nay, farther, as a strong *lixivium* of any of these salts acts much more powerfully in volatilizing ammoniacal salts, than lime-water, it ought, by the preceeding experiments and reasoning, also to dissolve the stone more quickly than this water; which however is by no means the case; for although a ley of potash renders the stone whiter, more friable, and somewhat rotten, yet it does not dissolve it*.

SINCE

* Vide Hales's Exper. on Mrs Stephens's medicines, and No. 62 above.

SINCE therefore fixed alkaline salts, which mix easily with oils, and powerfully volatilize the ammoniacal salt of the urine, shew very little power of dissolving the *calculus*, the principal and peculiar action of lime-water, whereby it so remarkably dissolves this concretion, must consist in its producing some change upon the principles of the stone different from what fixed alkaline salts are observed to do. But the only thing in which the action of quick-lime, and of fixed alkaline salts, upon *sal ammon.* and that of urine, differs, is, that while both seem equally to volatilize these salts, the former also destroys their power of effervescing with acids, and so changes their nature as to render them incapable of being reduced to a solid form. The virtue of lime, therefore, in dissolving the *calculus*, seems to proceed from that power which it possesses, not merely of rendering the salts in this concretion volatile, but of subtilizing and dividing them in such a manner as thoroughly to destroy their nature and texture as solid salts.

BUT further, since quick-lime seems to owe its virtues to its being deprived of its air by calcination, it is reasonable to think that lime-water dissolves the *calculus* partly by extracting the air out of it. No elastic air, however, is generated during the dissolution, because the lime-water extracts no more air out of the stone than itself is capable of absorbing.

SOAP-LEYS or a *lixivium* of potash and quick-lime, is a much more powerful solvent of the stone than either lime-water or a solution of potash in water; because the alkaline salt, after being deprived of its air by the quick-lime, becomes much more acrid and caustic than it was before, and not only acts more powerfully on the salt and air in the *calculus*, but also unites more readily with its oil.

WHEN a fragment of a *calculus* is immersed in soap-leys, there appear to issue from almost every point of its surface *striae*, or oily streams, which run through the ley, exhibiting much the same appearance that *alcohol* does when mixed with water. Are these *striae* owing to elastic air issuing from the surface of the stone, in order to unite with the alkaline salt of the ley, which has been deprived of

its air by the quick-lime; or are they rather owing to the oil and saline parts of the *calculus*, upon which this *menstruum* acts powerfully?

IT is observable, that while lime-water dissolves the stone, it does not affect biliary concretions, though much less firm and hard. The reason of which is, that the former abound much more with salt and air than the latter; and we have shewn above, that the lithontriptic virtue of lime-water is chiefly owing to its peculiar action upon the salts and air of the stone. But a *lixivium* of salt of tartar, although it has little influence on the *calculus*, dissolves the biliary concretions*; because the alkaline salt unites readily with oil, which is the principle that abounds most in them. If lime-water dissolved the *calculus* by its penetrating detergency, as some have thought, ought not biliary concretions to be dissolved by it, since they are more likely to yield to a penetrating detergent *menstruum* than urinary stones?

MR MORAND has remarked, that Mrs Stephens's medicines are not so successful in young as in old patients†; and I have observed the same thing with regard to lime-water and soap. At first I was ready to ascribe this to children's not taking these medicines regularly, and in the proper quantity; but, upon further consideration, I am inclined to think there is something more in it, especially since Dr Alston, who has of late published many curious experiments on lime and its water, informed me, that he had observed *calculi* extracted from children to dissolve more slowly in lime-water than those which were taken from old people.

CHILDREN, on account of the weak state of their digestive organs, and their food being almost wholly of the acescent kind, are remarkably subject to acidities in the *primæ viæ*. And if we may depend upon Mr Homberg's experiments, the blood of young animals affords a greater quantity of acid than the blood of old ones‡; nor can this acid be wholly deduced from the sea-salt in their blood, since in older animals the humours abound at least as much with this salt as in younger ones. It is not improbable, therefore, that in children,

* Hales's Statics, vol. 2. p. 192.

† Memoires de l'Acad. des sciences, 1741, edit. 8vo, p. 256. 268. and 269.

‡ Memoires de l'Acad. des. sciences, 1712.

children, the ammoniacal salt of the *calculus* may contain a greater proportion of acid than in old people. And as clays, or argillaceous earths, are said to be of no use in pottery when deprived of their acid, because they do not acquire a proper firmness and cohesion when burnt; so, perhaps, the stone in old people may be of a less firm cohesion, because their fluids are more destitute of an acid than those of children.

THE medicines, therefore, against the stone are probably less successful in young than in old patients; not only because their efficacy must be more weakened by the greater proportion of acid in the *primæ viæ*, blood, and other humours of the former, but also on account of the greater firmness and cohesion of the parts of the stone.

S E C T. XII.

Of the cure of the stone.

HAVING given an instance of the success of lime-water in dissolving the stone, and illustrated its virtue this way by a variety of experiments, in the course of which we have been so lucky as to discover the remarkable virtue of oyster and cockle-shell lime above stone-lime; it remains that a method of cure be laid down, founded upon the above history and experiments.

I. FIRST, then, I would advise the patient to swallow every day, in any form that is least disagreeable, an ounce of the internal part of Alicante soap*, and drink three English pints or more of oyster or cockle-shell lime-water. If he takes the soap in pills, or shaved down as Mr Millar did, he may divide it into three doses; the largest to be taken, fasting in the morning early; the second at noon; and the third at seven in the evening; drinking above each dose a large draught of the lime-water; the remainder of which he may take

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any

* The external part, which is discoloured by the air, is deprived, in a good measure, of its virtue. See No. 73. above.

any time betwixt dinner and supper, instead of other liquor *. The disagreeable taste of the lime-water may be blunted, by adding a little sweet-milk to it; and is quite destroyed, by washing one's mouth immediately after drinking it with a little vinegar and water; which, however, must be carefully spit out again. But, if the patient finds difficulty in taking the soap in this form, or if it does not sit easy on his stomach, let him dissolve † an ounce of it in an English pint and a half of warm lime-water, made with shells that have been long exposed to the weather, and take this at three different times, drinking the rest of the lime-water by itself. If it should happen, that shell lime-water cannot be had, then let him take the same quantity of stone lime-water, with at least an ounce and a half of soap; since its dissolving power is so much increased by it ‡.

It is observable, that a solution of soap in lime-water has not so disagreeable a taste, as a solution or decoction of it in common water.

THE soap is not only proper to be taken along with the shell lime-water, as it is endued with a considerable power of dissolving the stone, but likewise as it will destroy all acid humours in the stomach and guts, contribute greatly to keep the body open, and prevent any costiveness that might otherwise be occasioned by the lime-water.

THE patient should begin with a smaller quantity of the lime-water and soap than what is mentioned above; at first an English pint of the former and three drams of the latter, taken daily, may be enough. This quantity, however, he may increase by degrees; and ought to persevere in the use of these medicines (especially if he

* The least proper time for taking soap and lime-water, (as well as most other medicines), is immediately after a full meal, because in this way their virtue will not only be much weakened by being mixed with a large quantity of aliments mostly aced, but they will not be so readily received into the blood when the stomach and guts are previously stuffed with food, as when they are nearly empty.

† A solution is preferable to a decoction. See No. 56. above.

‡ See No. 66. above.

With regard to the quantity of soap to be taken by caldous patients, Dr Alston had judiciously remarked, that no more is to be used than keeps the body gently open; for, when it purges, what goes off this way cannot act on the stone. Dissert. on quick-lime, p. 25.

he finds any abatement of his complaints, or symptoms of the stone dissolving) for several months, nay, if the stone be very large, for years.

FURTHER, it may be proper for the patient (if he is severely pained) not only to begin with the soap and lime-water in small quantities, but also to use the second or third lime-water, instead of the first. However, after he has been for some time accustomed to these medicines, he may not only take the first water, but, if he finds he can easily bear it, heighten its dissolving power still more, by pouring it a second time on fresh calcined shells *. An English quart of this strong lime-water, with an ounce, or an ounce and a half of soap dissolved in it, and taken daily, would, I dare say, answer as well in calcalous cases, and be as little disagreeable, as any method yet proposed.

DURING a course of lime-water and soap for the stone, the patient should abstain from all acid and fermented liquors, as vinegar, wine, ale, beer, cyder, &c. For his drink he may have milk and water, whey, or a ptisan made with roots of althea, parsley, and liquorice: But, if he has been accustomed to more generous liquors, and cannot confine himself thus far, he may be allowed a little mountain Malaga, or some weak punch, made without any acid; but, as the virtue of soap is much weakened when dissolved in punch †, and entirely destroyed by spirits ‡; and as quick-lime has its nature considerably changed by them ||, they ought not to be drunk by themselves, not even in punch to any great quantity. It will also be proper to be sparing in the use of salt meats **, and honey ††, and to refrain from all fruits that have any acidity or sharpness ‡‡; while, on the other hand, milk and sugar ||| and animal food, with the vegetables mentioned N^o 39.—45. may be safely used.

It may be worth while here to take notice, that some who have advised Mrs Stephens's medicines, or soap and lime-water, for the
cure

* Vid. Sect. 3. No. 21. above.

† See No. 69. above.

‡ Hales's Experiments, p. 2.

|| See No. 1. & 2. above.

** No. 50.

†† No. 34. & 71.

‡‡ No. 36. & 38.

||| No. 33. & 34.

cure of the stone, have, at the same time, most injudiciously recommended Dr Lobb's diet for calculous patients——Dr Lobb has distributed our vegetable aliments into three classes, *viz.* those which have some considerable power of dissolving softer kind of gravel-stones, those which have a weaker dissolving power than the former, and those which have no dissolving power at all: but it is to be observed, that of the articles in the first and second classes, which are alleged to have some lithontriptic power, more than one half are of an acid or acedent nature, and consequently must greatly destroy the virtue of soap and lime-water; a few days use of which will communicate more of a dissolving quality to the urine than as many years perseverance in Dr Lobb's diet.

As the cure depends upon the urine being strongly impregnated with the virtues of the lime-water, the patient ought to drink no more of any other liquors than is necessary to quench his thirst, and should retain his urine as long as he can without uneasiness, that it may have the greater time to act upon the surface of the stone.

If, from catching cold, or too violent motion or exercise, the patient's pains should happen to be greatly increased, it will be proper to lessen, or even to abstain for a few days from his medicines, and to have recourse to opiates, emollient clysters, fomentations, and warm baths. If the soap and lime-water shall occasion a greater heat and thirst than usual, thirty or forty drops of dulcified spirit of nitre may be taken in a draught of lime-water twice or thrice a-day*.

If the lime-water should chance to occasion costiveness, it will be proper now and then to take a pill, of equal parts of aloes and soap, or any other of the purgatives mentioned N^o 51. 55.

If, instead of the soap, a quarter of an ounce of a ley made with purified potash and calcined oyster-shells, diluted with five ounces of lime-water, three ounces of sweet milk, and half an ounce of syrup of sugar, were swallowed twice or thrice a-day; it would probably be not more burdensome to the stomach, and certainly
would

* Vid. Sect. 10. No 75. above.

would contribute more to the speedy dissolution of the stone, as it is possessed of a much stronger lithontriptic virtue than the soap. And this ley is preferable to common soap-leys, as we have already observed; not only upon account of its being less nauseous, but also as it is a more powerful solvent of the *calculus* *.

BUT if we should meet with any patients who have an invincible aversion to soap in any shape, or to whom, upon account of ulcerations in the urinary passages, it would be improper either to prescribe soap or this alkaline ley; in such cases the above experiments give us reason to think, that oyster or cockle shell lime-water alone, drunk in large quantities, will have rather a greater effect in dissolving the *calculus* than stone lime-water, even when assisted by soap †: so that, in place of all Mrs Stephens's medicines, which to many delicate people can be of little use, we may substitute this lime-water with equal, nay perhaps greater success.

To render this still more probable, I need only relate the following experiment.

DR HARTLEY's mass of soap, lime, and salt of tartar ‡, which contains every thing valuable in Mrs Stephens's medicines, is by him ordered to be taken from three to four ounces a-day; and I have often prescribed lime-water from three to four English pints daily: To know therefore from which of the two medicines the greatest benefit is to be expected in the cure of the stone, I dissolved that gentleman's mass in sixteen times its weight of boiling water, and immersed in it a piece of B, of thirteen grains; and at the same time infused another fragment of B, of the same weight, in oyster lime-water, made with six pounds of water, to one pound of fresh calcined shells. After thirty-five hours warm and twenty-three cold digestion, the fragment in the lime-water had six grains and a half of its substance rotten and dissolved, while that in the solution of Dr Hartley's mass had only lost three grains.

DR

* See above, No [64.]

† Compare the experiments of the dissolving power of oyster and cockle shell lime water, No 14.—20. with experiments of the effects of a solution of soap in stone lime-water, No 66.

‡ See page 338. above.

DR HARTLEY, in his Latin epistle to Dr Mead, has proposed a variety of methods, in which powder of lime, soap, soap-leys, or fixed alkaline salt, may be taken for the cure of the stone: but as the following composition, which was communicated to me, at his desire, by Dr Hales, differs in some things from any of the *formulas* in his epistle, I shall here give it to the public.

“ TAKE Alicant soap shaved, eight parts, oyster-shell lime, one part; pour upon them a little water, and beat them into a soft uniform mass: then dissolve this mass into an emulsion, by adding more water, so as to make six quarts of emulsion from every pound averdupois of soap. Let this emulsion stand exposed to the air for a month in a wide-mouthed vessel, being stirred frequently and laved in and out of the vessels, as in cooling liquors. By this means it will become mild to the taste, stomach, and urinary passages. The dose is half a pint three times a-day. It may be called the alkaline emulsion for the cure of the stone.”

ALTHO' it is probable that this emulsion, like the solution of Dr Hartley's mass above mentioned, would dissolve the *calculus* out of the body more slowly than strong oyster-shell lime-water; yet it may perhaps produce as great or greater effects in the body, because it contains a certain quantity of lime not fully flaked, which must therefore communicate its virtues to all the humours it meets with in its passage thro' the stomach and intestines. However, as the safest, and least offensive, way of conveying the virtues of lime into the blood, is by drinking its water; and as this water, by being poured a second or third time upon fresh calcined shells, may have its strength and dissolving power increased*; I would still prefer it to the powder of lime, in whatever form. But, in cases where the lime-water and soap fail in giving the patient relief, the above emulsion may be tried.

SUCH as have no stone in the bladder, but are subject to frequent fits of gravel in the kidneys, might very probably prevent these, by drinking every morning, two or three hours before breakfast, an English pint of oyster or cockle shell lime-water; which though it might

* Vid. sect. 3. No. 21. above.

might be too small a quantity to have a sensible effect in dissolving the stone, yet would probably prevent any new concretions.

PATIENTS who have small stones in their kidneys, often pass (especially before a nephritic paroxysm) dark-coloured urine, very much resembling moss-water or coffee, and frequently feel some kind of dull pain or uneasiness in that part of their back where the kidneys lie. I have known some patients, who, after discharging this kind of urine for several weeks, have in two or three days voided 70 or 80 small stones, like pin-heads; after which they continued for a considerable time free of all gravelish complaints.

THE dark colour of the urine in these cases, is owing to blood mixed with it, which ouzes slowly, and in small quantity, from the vessels of the kidneys eroded by the rough surfaces of the stones lodged in them. This blood does not run into clots, because it is mixed with the urine gradually, and only in a small proportion, and it loses its red colour, by being retained a considerable time in the body, before it is evacuated. In like manner, blood ouzing slowly from the small vessels of the stomach, and lying there for some time, is vomited up of a dark coffee-colour, and often mistaken for black bile; whereas, when it flows in a greater stream, and from larger vessels, it is thrown up with its natural colour, either in clots or in a fluid form. The best remedies for this dark coloured urine, are mucilaginous drinks, such as Arabic emulsion, a decoction of *althea* root, or an infusion of lint-feed, which defend, in some degree, the vessels of the kidneys against the roughness of the small stones lodged in them; and lime-water, which, while it softens and renders smooth the surfaces of these stones, has, at the same time, a tendency to heal the eroded parts. Proper doses of *opium* are also useful, as they not only facilitate the expulsion of the small stones lodged in the kidneys, but lessen their power of doing mischief while they remain there.

II. IN order to render the cure of the stone in the bladder still more speedy, I shall offer a proposal which, how far it may succeed, is left to the judgment and experience of others.

It is this, that such persons as have a stone in the bladder, should, at the same time they are taking the medicines above directed, have four or five, or more ounces of tepid shell lime-water injected into their bladder every day, to be retained as long they can without pain; for which purpose they should evacuate their urine immediately before the injection. Were it not for the trouble of introducing the *catheter*, such injection might be made twice or thrice a-day; and if a flexible *catheter* could be always kept in the bladder *, it might be done as often as one pleased, and so the dissolution of the largest stones might be quickly procured. It may perhaps be proper to let the patient drink lime-water some weeks before he uses it by way of injection, in order to mitigate his pains, and take off that tenderness of the internal coat of the bladder, which generally attends this disease; after which he will, with less difficulty, be able to retain it when injected, so as it may have time to act on the surface of the stone.

THE injection of warm water into the bladder has often been practised in order to the high operation for the stone; and if then it was sometimes attended with bad consequences, this seems to have been owing to the great quantity injected, whereby the muscular fibres of the bladder, which resist such distension, were too much and too suddenly stretched: But in the present case (unless the stone be very large) so small a quantity will be sufficient, that if it be cautiously gone about, I do not see any hurt it can do. And as from the quantity to be injected, no bad consequences are to be feared †, so neither is it probable, that from the quality of the liquor any would arise: For lime-water, we see, may be taken into the

* Heist. Chirurg. p. 883. and 938.

† In Le Dran's Observat. 80. we find a decoction of marshmallows was injected into the bladder, morning and evening, for a considerable time, without the smallest inconveniency, and with remarkable good success in that particular case; which was, as he calls it, *vessie racornie*. To save the trouble of introducing the *catheter* twice, he let it remain in the bladder from the morning till after the evening injection was over. And in Dr. Hales's Statics, vol. 2. p. 212. we are told, that in four hours time he caused, by means of a double *catheter* there described, 900 cubic inches, or three English gallons of warm water, to flow in and out of a bitch's bladder in a continual stream, without the least harm or inconveniency to the animal, that he could perceive.

the stomach in great quantities without any harm; it is made use of to wash sores with very good success: And, as sometimes along with the stone there are small ulcers or excoriations in the bladder, the lime-water, either injected or taken by the mouth, will dispose them to heal, instead of having any of the bad effects which are to be feared from Mrs Stephens's medicines, which render the urine highly alkaline; and of which I had, some years since, occasion to see an instance in a patient, who having several symptoms of a stone in the bladder, had frequently taken a good deal of soap, by which his pains, especially in making urine, were always so greatly increased, that he was obliged to lay aside the use of it; but, upon inquiring more narrowly into his case, I found he had, along with the stone, an ulcer in his bladder, and passed considerable quantities of purulent matter. Agreeably to this, Mr Morand has observed, that in such cases where, along with the stone, there were any ulcerations in the bladder, Mrs Stephens's medicines always occasioned great pain and uneasiness*; while, on the other hand, Dr Langrish found, that, when, by injecting too great a quantity of soap-leys, he had made dogs void blood with their urine, lime-water would immediately be retained in the bladder without any uneasiness, and quickly heal the small vessels, which the acrimony of the soap-leys had eroded†.

THAT the injection of lime-water into the bladder may be rendered more safe, and attended with less uneasiness, two scruples or a dram of starch may be dissolved in six or eight ounces of oyster lime-water, and just brought to boil over the fire, stirring it all the while: For having put a piece of B, seven grains, in such a mixture as this, in three hours time, there was a white rotten crust formed all round it, which fell off upon shaking the glass, and, in twenty-four hours time, above a grain of it was dissolved. The heat used in this experiment did not exceed 100 degrees in Fahrenheit's thermometer.

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* Memoires de l'Acad. des sciences, an. 1740.

† Physical Experiments, p. 19.

THE fourth part of the yolk of an egg being mixed with six ounces of lime-water, does not weaken its virtues any more than the starch, and may occasionally be used in place of it.

I tried also *gum. Arab.* and *sem. lini*; but they both destroyed the virtue of lime-water more than the starch or egg.

I was very much pleased to find, that the proposal I had made in the first edition of this essay, of injecting lime-water into the bladder, with a view to the dissolution of the stone, had engaged the ingenious Dr Langrish to pursue this matter a good deal further. This gentleman, in his Physical experiments upon brutes, published in 1746, has shewn, that the bladders of dogs can not only bear stone and oyster shell lime-water injected twice a-day for a month, without any pain or ill effect following, but lime-water mixed with soap-leys, in the proportion of fifteen, twenty, or twenty-five drops of the latter, to each ounce of the former; provided a little starch be added to blunt the acrimony of the soap-leys.

IN the year 1745, Mr John Campbell, late surgeon in this place, at my desire, injected near two ounces of oyster-shell lime-water, in which a little starch was dissolved, into the bladder of a boy about ten years of age, who had been taken into the Royal Infirmary to be cut for the stone. We directed him to empty his bladder before the injection was made, and he retained it without any pain or uneasiness for near three hours.

MR CAMPBELL after this injected, into the bladder of a man, lime-water unmixed with any thing to soften it; which, however, gave no pain nor uneasiness, although retained a considerable time. So that from these trials upon the human body, and Dr Langrish's experiments upon dogs, it appears, that the only thing wanting in order to the dissolution of the stone by injections, is an easy way of conveying these into the bladder; for the introducing a common *catheter* twice or thrice a-day would necessarily give a good deal of uneasiness, and soon fret the parts; and it is not easy to contrive a flexible *catheter* that could be always retained in the bladder, without considerable inconveniency. I have for some time entertained an opinion, that, if a syringe was made with a small
pipe

pipe joined to it, which might be introduced three or four inches into the *urethra*, and the *penis* grasped pretty firmly, so as that it should closely embrace the pipe of the syringe, a liquor might be pushed with such force into the *urethra*, as to overcome the resistance of the *sphincter vesicæ*, and make its way into the bladder, without doing any harm, or giving much pain*. And in this opinion I have been confirmed by a gentleman, who was some years ago my patient; for he assured me, that he had frequently, without any *catheter*, forced the *sphincter*, and thus injected lime-water into his bladder, for the cure of an ulcer there. Nor do I apprehend, that an injection forced into the bladder, in this way, would be in any danger of penetrating into the excretory ducts of the *prostatæ*, or *vesiculæ seminales*, before it overcame the natural contraction of the *sphincter*.

BUT as some of my friends, for whose judgment I had a great regard, seemed to be of opinion, that the *sphincter* of the bladder could not be forced in the way just now mentioned, I did not prosecute the thing any farther; nor did any one else take the hint, from the year 1747, that I first published this proposal †, to June 1752, when, in order to be fully satisfied in this matter, I desired Mr William Butter student of physic to try whether he could not inject a liquor, in the manner above proposed, into the bladders of some of the patients in the Royal Infirmary; in order to which, I directed him to get an ivory pipe made of about 4 inches in length, and of such a diameter that it might easily enter the *urethra*, and to mount it with a bladder in the same way as is done to a clyster-pipe.

IN consequence of this, Mr Butter procured an ivory-pipe of about $4\frac{1}{2}$ inches in length, whose diameter was $\frac{1}{8}$, and its bore $\frac{1}{10}$ of an

* Or perhaps it might do better if there was an ivory pipe made five or six inches long, and of the size of a common *catheter*, with a sheep's bladder tied upon its great end, as is usually done in clyster-pipes: For if the injection was put into this bag, and the pipe introduced into the *urethra*, it could be much more equably forced into the bladder, than by a syringe, which is apt to jerk, and is far from being so much at one's command. See Langrish's Experiments, p. 51.

† Vid. Edinburgh Medical Essays, edit. 3. vol. 5, part 2. p. 228.

an inch: upon the great end of this pipe, which was formed like a common clyster-pipe, he tied an ox's bladder; and, having put four or five ounces of warm milk and water into it, introduced the pipe near four inches into the *urethra* of Thomas M'Curfy, aged nineteen; then directing him to grasp his *penis* strongly with his hand, he pushed forward the injection with a good deal of force; but, thro' a neglect of some of the directions to be mentioned below, little or none of the liquor penetrated into the bladder. However, after a few unsuccessful trials, he pushed first four ounces of milk and water, and afterwards four ounces of milk, into the bladder of this patient, without giving him any pain or uneasiness.

BUT as this experiment did not succeed quite so well when I was present, I ordered a pipe to be made of the same diameter with the one above mentioned, but of 7 inches in length, thinking that, by this means, the injected liquor might be made to act with more force upon the *sphincter* of the bladder. As a piece of ivory could not be procured 7 inches long, the pipe was made of tin. Upon the great end of it a bladder was fastened; into which having put five ounces of stone lime-water, with half that quantity of milk, and firmly tied the bag, Mr Butter introduced the pipe into the *urethra* of the before-mentioned patient, (till, with my finger, I felt the point of it within less than an inch and a half of the *anus*), and endeavoured to push forward the injection by strongly pressing the bag, which, being too weak, burst before almost any thing had got into the bladder. But, having afterward procured a stronger bag, he injected, between the hours of two and eight in the afternoon of this day, four different times, into the bladder of Thomas M'Curfy, five ounces of tepid oyster-shell lime-water, unmixed with any thing. The injection was generally performed in a minute, sometimes a good deal sooner. The same afternoon, viz. June 30. 1752, he injected five ounces of lime-water into the bladder of Thomas Sanderson, another patient in the Infirmary, aged 30, in the space of forty seconds. These patients neither complained of pain when the pipe was introduced, the injection pushed, nor when they voided the lime-water; only M'Curfy said, he perceived a little more *stimulus* than when

when he used to make urine, but not so much as to give him any uneasiness worth mentioning. They had no inclination to empty their bladder immediately after receiving the injection, and were able to retain it without any difficulty. Altho' the lime-water injected was perfectly limpid, yet when they voided it, after having been retained some time in the bladder, it had a turbid appearance, and looked as if a few drops of milk had been added to it. That this change of colour in the lime-water was owing to the urine mixed with it, during its stay in the bladder, is evident from sect. 2. No. 11. where it is shewn, that lime-water, when mixed with urine, becomes whitish and turbid.

JULY 6. Mr Butter, when I was present, injected with the short pipe, near five ounces of milk and water, in little more than half a minute, into the bladders of the same two patients, notwithstanding one of them had, at the time the injection was made, a little of a strangury from a blister on his head. Whence it appears, that the length of the pipe is not a circumstance so material as, at first, I was apt to imagine.

IN order to make the injection of lime-water into the bladder succeed better, it may be of use to attend to the following directions, which were observed by Mr Butter in most of the above experiments.

1. THE patient ought to empty his bladder immediately before the operation.
2. HE should lie in bed upon his back, with his legs drawn up to his body, and his thighs asunder.
3. HE should be forbid to restrain respiration, but desired to breathe in his usual way. He must likewise be put on his guard, to give no resistance to the injection, when he feels it entering into his bladder, and to restrain any attempt to make urine, altho' he should, at that time, have a small inclination to it.
4. THE liquor to be injected should be blood-warm, or nearly so.
5. THE bladder fixed to the pipe must be strong, and very firmly tied, otherwise it will be apt to burst, or to allow the injection to escape by the ligature.

6. THE

6. THE pipe should be dipt in oil before it is introduced into the *urethra*.

7. AFTER the pipe is introduced, the patient must compress his *penis* very firmly with his hand, else the liquor, instead of making its way into the bladder, will return by the *urethra* towards the point of the yard.

8. THE bag containing the liquor to be injected must be pressed by one or both hands of the operator, with a very considerable force, in order to dilate the *sphincter* or neck of the bladder.

9. AT first the lime-water may be softened, by mixing it with a little milk or starch, and only four ounces of it injected; but afterwards the quantity may be increased to five or six ounces, and double lime-water may be used *.

10. As, from the principles of hydrostatics, it is evident, that the force required to dilate the *sphincter* of the bladder must be, *ceteris paribus*, proportional to the surface of the liquor contained in the bag, no more should be put into it than is intended to be thrown into the bladder. The not attending to this circumstance, was not only one reason of the bag's bursting, but also of the injection's not passing so easily into the bladder, in some of the first experiments, which were made with the pipe of $4\frac{1}{2}$ inches in length.

IT may be worth while to observe, that, as one can, at pleasure, some how relax, in a small degree, the *sphincter vesicae*, a patient would probably, after a few trials, learn to push the liquor precisely at the time of the relaxation, whence he would make the injection with more success upon himself, than the ablest surgeon could do upon another. But as more force may be sometimes required than he can apply with one hand, Mr Butter is of opinion, that, if the liquor to be injected were put into a pair of small bellows contrived for the purpose, and the pipe screwed to their nose, the patient could with little or no difficulty bring the sides of these bellows together, so as to push the fluid contained in them into the bladder with great force.

IN

* Vid. Sect. 3. No 21. above.

IN the other sex, whose *urethra* is streight and much shorter than in men, the pipe above described may be introduced into the bladder itself without giving any pain, and so the injection be pushed into it with the greatest ease.

IF five ounces of oister-shell lime-water were thus injected into the bladder of either sex, at seven or eight in the morning, at noon, and at six in the evening, and retained two or three hours each time, there can be no doubt but that the stone must as certainly be dissolved in this way, as if it were immersed in a phial full of lime-water, though indeed not so soon. And in women, who may be easily taught to make the injection themselves, the stone must, in time, be so certainly dissolved, that, for the future, it will only be necessary for them to have recourse to the operation of lithotomy in very rare cases, where the stone is of such hardness as to resist the force of the oister-shell lime-water.

FURTHER, as the severe stimulating pains, which generally accompany the stone in the bladder, are not so much owing to its bulk, as to the roughness of its surface, the oister-shell lime-water injected, in the manner above described, twice or thrice a-day, would not fail, in a very short time, to give certain relief from those pains, by dissolving the sharp points of the stone, and converting its surface into a soft chalky substance.

HOWEVER, it is to be observed, that while lime-water is thus injected into the bladder with a view to the dissolution of the stone, it ought also to be drunk to the quantity of an English quart at least, and an ounce of Alicant soap taken along with it, every day: As this will not only destroy that quality in the urine whereby it generates and increases the stone, but will communicate some degree of a dissolving power to it, and consequently hinder it from weakening the virtue of the lime-water injected into the bladder so much as it would otherwise do.

WHEN the *sphincter vesicæ* cannot be easily forced in the manner which I described above, or by means of Mr Butter's bellows, lime-water may, without any difficulty, be conveyed into the bladder by means of Mr Daran's newly invented hollow *bougie*; which has

this advantage over a *catheter*, that it may not only be introduced into the bladder with little or no pain, but may be retained there for a long time, without giving any considerable uneasiness to the patient.

S E C T. XIII.

The comparative value of the several medicines proposed for dissolving the stone.

THAT the method of cure just now proposed may appear with greater advantage, I shall state in a few words the comparative value of the several medicines that are thought to bid fairest for dissolving the stone. Of these only lime-water and soap can be taken safely into the human body. Spirit of nitre, spirit of sea-salt, soap-leys, or the fiery *lixivium* of No. 64. and quick-lime, being all poisons, are only capable of being used when diluted in a large quantity of some watery vehicle. And even then they do not promise so much as lime-water: for as the virtue of spirit of nitre seems to consist in its extraordinary corrosive acidity, which must be in a good measure destroyed before it gets into the blood, and still more so before it arrives at the bladder; nothing can reasonably be expected from it, or any medicines of this kind. I know it has been alledged, that although vegetable acids are entirely changed by the digestive powers of the human body; yet this is not the case with respect to mineral acids: To which purpose the learned Boerhaave is quoted; who, in his chymistry, observes, that such acids as are fit to dissolve gold, silver, &c. are generally too strong for the concoctive powers of animals, and hence become poisons. But this authority is as strong as any thing can be against those who use it: For, if the mineral acids above mentioned are entirely changed by the powers of the human body, it is confessed they can have no effect in dissolving the stone; and if they are not destroyed, they become poisons, and consequently cannot safely be exhibited with a view to the dissolution of the *calculus*.

SPIRIT

SPIRIT of sea-salt is liable to the same objections as spirit of nitre; at the same time that it is not near so powerful a solvent of the stone *.

WITH regard to soap-leys, or the caustic *lixivium* of No 64. it may be observed, that as they owe a great deal of their destructive quality to an ingredient that has scarce any effect in dissolving the stone †, they do not seem so well calculated for this end as lime-water; which, at the same time that it is strongly saturated with that principle to which the soap-leys owe their virtue, is free of the alkaline salts, which renders them in a great measure so noxious. But to set the virtue which these two medicines have of dissolving the stone still in a clearer light, it may not be improper to compare the effects which soap-leys had on Dr Jurin, with what we have seen of lime-water in the case of Mr Millar.

DR JURIN'S stone in his bladder seems to have been but of two or three months standing when he began his medicines; whereas Mr Millar's was of above fifteen months. Dr Jurin took soap-leys in very large doses for near five months before he passed any stones; and, after taking them near seven months, does not seem to have been perfectly cured ‡: Mr Millar, in seven weeks after he began to drink lime-water, voided one stone, as in three months he did another, and has ever since been perfectly well. Mr Millar had no pain upon using the lime-water ||, but in few days began to perceive a gradual abatement of all his complaints; while Dr Jurin had his pains considerably increased by the soap-leys at first; nor does he seem to have had any sensible ease, till after using them above four months.

By what has been just now said, I would not be thought to reject the use of soap-leys altogether in the cure of the stone; on the contrary, I imagine, that small doses of this liquor, taken along with lime-water, and milk §, may contribute greatly to the dissolution of the *calculus*: Only I would not chuse to give it in such

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large

* See No 76. above.

† See No. 76. above.

‡ See his case, p. 14.

|| Nor is Mr Millar a single instance of this: For of all the patients for whom I have ordered lime-water in the stone, I do not remember that one had his pains increased by it.

§ See Sect. 11. No. 1. above.

large quantities as to occasion great pain or heat of urine to the patient, or in any case where there are ulcerations or sores in the bladder. And, instead of common soap-leys, I would recommend the *lixivium* of [N^o 64.] made with purified potash and shell-lime, for the reasons there offered.

QUICK-LIME, as we have already observed, has been a long time looked upon by the chymists, as containing in it a powerful remedy against the *calculus*. The powder of calcined egg-shells, which makes a principal part of Mrs Stephens's medicines, is commended by Barbette as of incomparable use in all suppressions of urine from the stone or gravel *: And the ashes of burnt snails, another ingredient in her medicines, are mentioned near seventeen hundred years since, by Pliny, as a good remedy against the stone †. But, from the above experiments, it seems reasonable to think, that the virtues, whether of stone or shell-lime, may be more safely conveyed into the blood by means of lime-water than any way else; for lime, fresh from the fire, is too acrid and corrosive to be taken into the stomach; and, after it is flaked by two or three months keeping, as Mr Stephens directed, it gains in mildness only what it loses in virtue ‡; though, even after all, it is a medicine which seems to be neither innocent nor agreeable.

SOAP is not only inferior to lime-water, as its lithontriptic virtue is less, but as it abounds with an alkaline salt, which, when the soap is taken in large quantities, occasions great pains in the urinary passages, and is of bad consequence if there are any ulcerations in these parts. Nay, the stimulating quality of soap is so remarkable, that although lime-water, with a small proportion of soap-leys, softened with a little starch, gave no uneasiness when injected

* "In omnimodo urinæ a calculo suppressione, testæ ovorum calcinatæ, ad scrup. ii. vel drach. i. assumptæ, omnia reliqua medicamenta præcedunt." Barbette Praxis, lib. 4. cap. 8.

† "Easdem (sciz. cochleas) exemptas testes tritasque tres in vini cyatho bibi, sequente die duas, tertio die unam, ut stillicidia urinæ emendent: TESTARUM VERO INANIVM CINE-
"REM AD CALCULOS PELLENDOS. Plin. Hist. natural. lib. 30. cap. 8."

‡ See No. 20. above.

jected into the bladder of dogs: yet a small quantity of soap dissolved in the same lime-water never failed, soon after being injected, to cause great irritation; nor was the starch of any avail to prevent this *.

BUT, further, how strongly soever several of the above mentioned medicines may be endued with a virtue of dissolving the stone; yet lime-water seems to have this incontestible advantage, that it may be injected most safely into the bladder; and by being applied immediately and unchanged to the very surface of the *calculus*, it may be well expected to hasten its dissolution very considerably.

It has been alledged by some, that lime-water alleviates the patient's pains, by covering the surface of the stone with the calcareous matter which it contains, and consequently rather increases than lessens its bulk: and that it crusts the stomach all over with a coat of lime; and therefore must be a pernicious medicine when taken in large quantities, and used long.

IN answer to these objections, it is sufficient to observe, that a large quantity of lime-water contains only a very small portion of earthy matter †, and that it does not deposite one grain of this matter as long as it is kept close from the external air. Since therefore the urine, while in the bladder, has as little communication with the external air, as if it were in a bottle hermetically sealed, the lime water, which makes a part of it, can let fall no powder to cover the surface of the stone. Add to this, that lime-water mixed with *sal ammoniac*. or urine, neither throws up a scum, nor lets fall a calcareous sediment, even when exposed to the air ‡. Indeed, as lime-water changes the calculous matter in the urine into a light white sediment; if this should be supposed to cover the surface of the stone, it would, in some measure, defend the bladder against its sharp points.

It may be thought that lime-water, perhaps, lets fall part of its calcareous matter in the stomach, which is not always shut, but has a communication with the external air as often as we swallow: this,

* Langrish's Physical experiments, p. 30.

† See page 348. above.

‡ See page 364. above.

this, however, seems to be prevented by the little access which the air has to the stomach, the short stay which the lime-water makes in it, and the mixture of the different things it meets with there. Further, as lime-water becomes effete, as soon as it loses its earthy matter, if it were deprived of this in the stomach, it would become ineffectual in the cure of the stone. But experience, which is the most satisfactory proof of the innocence of any medicine, shews us, that lime-water may be used daily in large quantities, and persisted in for years, without injuring the health, impairing the appetite, or weakening the digestion; nay, it often mends all these. This was the case with the late Lord Walpole of Woolterton, who, after he had used lime-water and soap above three years, wrote me, that they were so far from hurting him in any respect, that they had given him a better appetite, and improved his state of health.

ALTHOUGH there are few urinary stones so hard as to resist oyster-shell lime-water out of the body, yet, as the virtues of this medicine, as well as soap, are greatly weakened, by their being mixed with the whole mass of blood, before they arrive at the bladder, it is no wonder if they make but small impression on the harder stones, and only dissolve such as are of the softer kind. However, even in cases where they are not able to accomplish an entire dissolution, they generally give surprising ease to the patient, and at least prevent the farther growth of the stone. They produce the last effect, by destroying the petrifying quality of the urine*; and the first, by wearing off the sharp points and rougher parts of the surface of the stone, which use to prick and irritate the tender membrane of the bladder: At the same time, it is not improbable, that the small part of the stone, which is dissolved by the medicines, may, partly, remain on its surface, in the form of a white chalky powder, as happens to gravel-stones immersed in lime-water out of the body

BUT,

* Vid. Sect. 2. No. 11. above.

BUT, as it would betray no small weakness to believe, that lime-water and soap always dissolve the stone when they relieve the patient; so it bespeaks a mind greatly under the influence of prejudice to deny that these medicines ever dissolve this concretion; since there have been many instances of patients, who, by the use of them, have passed calculous fragments at different times, for months, nay, years together. However, as it may be alledged by some, that these fragments were not part of a larger stone, and so no proof of the dissolving power of the soap and lime-water; I shall here mention the case of a patient of my own, where the contrary was evident beyond all manner of doubt. The Reverend Mr J. L. a clergyman of the church of Scotland, who had been much afflicted with the stone in his bladder, not only found himself greatly relieved of his former complaints, by swallowing daily, for a few months, an ounce of Alicant soap, and drinking near three English pints of oyster-shell lime-water, but passed a vast number of calculous fragments of different sizes; the largest was $\frac{1}{5}$ of an inch in length, and near $\frac{1}{4}$ of an inch in breadth. Their surface was partly covered with a white chalky crust. All of them were thin and appeared evidently to be *strata*, layers, or coats thrown off a large stone; especially the large pieces, which were remarkably convex on one side, and concave on the other. An instance not unlike this we have recorded by the late celebrated Dr Mead, in the following words: “*Medicus quidam Londinensis, mihi amicissimus, hac ipsa medendi via mercatorem graviter laborantem eripuit; plurima enim frustula, nunc crustularum, nunc exiguum nucleorum instar, simul cum urina foras ejiciebantur*”.

UPON the whole, we have found in lime-water, particularly in that which is made with oyster or cockle shells, a *menstruum* for the *calculus*, so innocent and mild, that it may be taken into the stomach without any harm, and injected into the bladder without the least danger of corroding it. Such a *menstruum* as this the learned Boerhaave did not despair of being one day discovered, as he had found
the

* *Monita et præcepta medica*, p. 178.

the spirit of rye-bread possessed of a surprising power to dissolve certain stones, though it did not injure any parts of the human body *.

I don't know whether it will be thought worth while just to observe, that lime-water appears, from the above experiments, to be possessed of all the qualities which Van Helmont required in a medicine that should safely dissolve the stone, *viz.*

1. "APTUM sit in urinam mutari, ut scilicet locum affectum tangat." That lime-water alters the nature of the urine, and communicates its virtues to it, appears from exper. under sect. 2.

2. "HABEAT in se potestatem solvendi repagula calculi." See exper. under sect. 2. and 3.

3. "POSSEDEAT istud in proprietate specifica."

4. "SIT subtile, ut quaquaversus eat, suumque eminus objectum demoliri queat." See sect. 9. N^o 57. 59. and 61.

5. "AMICUM sit naturæ, ne scilicet cuncta pervertat." See sect. 3. N^o 19. and sect. 9. N^o 61.

S E C T. XIV.

The dissolving powers of the menstrua.

IF, in the foregoing experiments, the weights of the several pieces of the same *calculi*, and the times they were immersed in the different *menstruums*, had been all equal, the strengths of the *menstruums* would at first sight have appeared; but since the case is otherwise, and I did not advert to this inconveniency arising from it, till it was too late; in order to remedy it as much as possible, I shall here subjoin a table of the proportions which the dissolving powers of *menstruums* for the *calculus* in the above experiments bear to one another; but, previous to it, I shall briefly mention the grounds upon which it is built.

IF the weights, and consequently the surfaces of two similar pieces of the same homogeneous *calculus* be equal, and the times which

* Element. Chem. vol. 1. de menstruis.

which they are immersed in different *menstruums* be also equal, the powers of the *menstruums* will be directly as the quantities dissolved.

If the weights and quantities dissolved be equal, the powers of the *menstruums* will be inversely as the times during which the *calculi* were immersed.

If the quantities dissolved, and times of immersion of two similar pieces of the same *calculus* be equal, the powers of the *menstruums* will be inversely as their surfaces, and consequently as the squares of their diameters, or the squares of the cube-roots of their weights.

THEREFORE, when the times, weights, and quantities dissolved are unequal, the powers of the *menstruums* will be directly as the quantities dissolved, and inversely as the times and squares of the cube-roots of the weights of the *calculi*. Thus, supposing M, m, to be the power of the *menstruums*, Q, q, the quantities, T, t, the times, and W, w, the weights of the *calculi*; then $M : m :: Q \times t \times \sqrt[3]{W}^2 : q \times T \times \sqrt[3]{w}^2$.

If the same *menstruum* be employed in dissolving similar pieces of different *calculi*, when the *calculi* are of the same hardness, the times required to accomplish their total dissolution will be directly as their diameters. When the diameters are equal, the times will be directly as the hardnesses. Wherefore calling H, h, the hardnesses, T, t, the times of total dissolution, and D, d, the diameters. $T : t :: D \times H : d \times h$. Whence $H : h :: \frac{T}{D} : \frac{t}{d}$ i. e. $\frac{T}{\sqrt[3]{W}} : \frac{t}{\sqrt[3]{w}}$

T A B L E.

<i>Menstruums.</i>			<i>Dissolving powers.</i>
Stone lime-water, experiment 11.	-	-	100
Strong stone lime-water, exper. 12.	-	-	130
Oister-shell lime-water, exper. 16.	-	-	296.
Stone lime-water in cold digestion in the month of May, exper. 13.	-	-	49
	H h h		Oister-

<i>Menstruums.</i>	<i>Dissolving powers.</i>
Oister-shell lime-water in said cold digestion, exper. 18.	- 124
Lime-water made with oister-shells, which had been exposed to the air for 35 days after calcination, exper. 20.	- 112
A solution of soap in common water, exper. 70.	- 75
A solution of the internal part of soap in ditto, exper. 73.	108
A solution of the external part of soap in ditto, exper. 73.	40
A solution of soap in stone lime-water, exper. 66. first trial,	195
A solution of soap in ditto, exper. [66.]	- 150
Oister-shell lime-water and sugar, exper. 35.	- 184
Oister-shell lime-water and honey, exper. 34.	- 79
A solution of soap in common water with sugar, exper. 71.	67
A solution of soap in water with honey, exper. 71.	- 34
A ley of oister-shell lime and potash, exper. [64.] p. 396.	3112
A stronger ley of the same kind, p. 395.	- 3890
Glasgow soap-leys, p. 390. 391.	- 1945

P O S T S C R I P T.

Edinburgh, Nov. 9. 1742.

AS I conceive the following history, incomplete as it is, may serve considerably to recommend the use of lime-water in the stone, I have sent it to be published (if you think it deserves a place in your collection) as an appendix to a former paper upon this subject.

JAMES LITSTER, of Macky's mill, in the county of Fife, aged fifty-seven, about nine years ago, was much hurt by a severe fall upon a mill-stone; and, after that, began to be troubled with gravel in the kidneys, and *ureters*. At this time he had a great pain in one of his kidneys and stoppage in his belly, like an iliac passion. After a clyster, he had passage, and was easier, and the stone or stones passed into his bladder: But he did not observe that he voided any with his urine, till a long time after; and has ever since had all the symptoms

symptoms of a stone in the bladder. He has great pain in making urine, is not able to use exercise, and, upon motion, feels the stone pricking him in his bladder. Riding at a trot gives him remarkable uneasiness; and after it, or much walking, his urine is generally tinged with blood.

HE has been subject for several years to have his symptoms, at certain periods, exasperated. During the fit, he is in great distress; and his urine, which he has an inclination to make every two or three minutes, comes away in drops, with severe stimulating pains.

THE fits generally last three, sometimes four weeks, and return after an interval of fourteen or twenty days.

HE took Mrs Stephens's medicines for two months without any benefit; but found his pain increased by them, his stomach put out of order, and appetite greatly destroyed.

HE has also used soap for some time, to the quantity of three-fourths of an ounce a-day; but without any sensible benefit.

I advised him, along with the soap, to drink lime-water made with calcined cockle-shells, beginning with two English pints; and if it agreed with him, to increase the quantity to three pints or more every day.

UPON the 1st of June 1742, a few days before he began to use the lime-water, he was attacked with a severe fit of pain, and difficulty of urine; which lasted twenty days; in which time he passed a good deal of tough slime. But, in eight or ten days after this was over, he found himself easier than he had been for a year before, and made his urine more freely, and with less pain.

JULY 2. Having used the lime-water scarce four weeks, he rode fourteen miles to a market. During the riding, he felt some pain in his bladder. Notwithstanding which, next day, he was quite easy; whereas, formerly, if he had made any journey on horseback, he was sure the day following to be in the utmost distress, and to continue so for seven or eight days.

WHEN he stoops down, or makes any sudden motion, he still feels the stone pushing him in the bladder; but not near so sharp as usual. His urine, since he used the lime-water, deposits a great

H h h 2 deal

deal of whitish sediment; and he thinks it has given him a better appetite than he has had for several years past.

FROM the 2d of July, he drank above three pints of lime-water every day; and was very easy till the 20th of that month, when he had a fit as usual; but it lasted only eleven days: and then he had only pain in making his urine; which, however, was less severe than what he had formerly been used to have. His belly being generally costive during the fit, I ordered him an infusion of *fenna*; from which he had considerable relief.

HE continued very well all the month of August; and walked, upon the 20th of that month, six miles in a few hours, without any pain or trouble: whereas, for some years past, he could not walk even one mile without much pain.

UPON the 1st of September, he found his urine much obstructed, and had a fit which lasted nine days: But, though his provocations to urine were frequent, and the difficulty in making it considerable; yet, after voiding a few drops, he was easy, and had no stimulating pains. From this time, to the end of October 1742, he was as well as if he had had no stone, was able to go about his business, from which he had been long laid aside, and was fit for any work that did not require great strength. He can ride now as well, either at a trot or gallop, as ever, and never finds any thing pricking him in his bladder, not even when he has the fit. From the beginning of July he made no use of soap, and found no other inconveniency, than that he thought the lime-water bound his belly a little more for the want of it. He is so sensible of the benefit he has had by drinking lime-water, that he is resolved to persist in it in hopes of a compleat cure *.

E

* From October 1742, James Littler continued drinking the lime-water near two years, to the quantity of three English pints a-day; during which time he passed great plenty of sandy stuff, was remarkably easy, and went about his ordinary business without any pain or interruption. After September 1744, he gave over the constant use of it: But, when after catching cold, or great fatigue, he had difficulty of making his urine, with some pain, and passed a good deal of slime, with some sand; upon having recourse to the lime-water in smaller quantities, he was soon relieved. Since the year 1744, he has, upon motion, felt nothing weighty like a stone in his bladder, (as he used formerly to do), although he has often walked ten miles a-day, made pretty long journeys on horseback, and frequently rode at a gallop.

So

I have had occasion to order oyster-shell lime-water for many other patients afflicted with the stone; but have not yet met with an instance, except one, where it was drank to the quantity of three English pints a-day, and any tolerable regimen observed, without the patient's finding himself, in a few weeks, sensibly easier *. When the stone is small, this will happen most remarkably, as was the case with Mr Millar; but, if it is very large, even after its surface is softened, and the rough points worn off it by the lime-water, it is not to be imagined, but that by its bulk it must sometimes give uneasiness, especially in making urine: but the patient has this to comfort him, that while he continues his medicine, the stone is daily growing less. And there is one sure mark by which any one taking lime-water, soap, or soap-leys, for dissolving the stone, may know if his urine is so far changed as not to be further capable, at least of furnishing any new matter for increasing its bulk, viz. if, by the use of these medicines, the sediment of his urine, from a brownish colour, becomes white.

I shall only make one observation upon this history, which the most rigid infidels with regard to the dissolution of the stone must acquiesce in; and that is, supposing lime-water could not dissolve the stone; yet, since it is capable of giving such ease and quiet to those who labour under it, whether would it not be more adviseable for such, especially if advanced in years, to resolve upon drinking a bottle of this every day, during their whole life, than submit to one of the most cruel, and, at the same time, not the least dangerous operations in surgery?

A P P E N -

So that it seems probable, that the stone, if yet undissolved, is at least considerably diminished in its bulk, and softened in its surface. It may be worth while to observe, that this patient, notwithstanding his drinking cockle-shell lime-water for above two years, to the quantity of three English pints a-day, had neither his appetite nor digestion any way impaired, nor his health injured by it.

* If there is, along with a stone, any ulceration in the bladder, as sometimes happens, the lime-water will often fail in giving relief; and yet it seems, even in this case, to be the best remedy we know of, especially if it is not only drunk in large quantities, but also injected daily into the bladder.

A P P E N D I X

CONTAINING

The C A S E S of

THE RIGHT HONOURABLE

HORACE WALPOLE, Esq; afterwards Lord WALPOLE.
of Woolterton,

T H E

Reverend Dr NEWCOME, Canon of WINDSOR, afterwards
Lord Bishop of Llandaffe,

WILLIAM HAY, Esq;

Mr L. TREVIGAR,

&c.

A. B. C. D. E. F. G. H. I. J. K. L. M. N. O. P. Q. R. S. T. U. V. W. X. Y. Z.

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I. The CASE of the Right Honourable HORACE WAL-
POLE, Esq;

Written by himself.

ABOUT eighteen years ago, when his Majesty resided at Hampton-court, I was taken ill with what was thought to be a fit of the cholic only, being subject to that disorder when I was very young; and the physicians treated me accordingly: When some days after, I was got perfectly well, in making water one morning, I voided a stone in the pot about the bigness of a barley-corn, which, without doubt, had occasioned, while it lay in the *ureter*, the cholical pain I had felt. From that time, I was frequently troubled with severe fits of the same pain, which lasted until, by turpentine-clysters, and other lubricating medicines, I had brought away a stone. Being advised at last to drink a pint of whey, made with cream of tartar, every morning; and having followed that method, from the beginning of May to November, at the end of two years, (during which time my pains frequently returned and ended in the same manner), I found myself perfectly cured: For, having persisted in drinking of whey yearly, I continued free from those pains, voiding only at times some red gravel till 1747. In the spring of that year, while I was at a friend's house in town, to dine there, having need to make urine, I made instead of it what appeared to be almost clear blood; and so, from time to time, for almost all that year, I was often called upon to make water, by very short intervals, which was more or less discoloured, seldom very clear, and frequently attended with great pain and some gravel. That whole year, until the next spring, I took variety of things of a lubricating and cooling nature, which it is unnecessary to detail, without any good effect. The next winter, in town, I found I grew daily worse, and although I did not always make bloody or coffee-water, yet my provocations to urine (which, after a hasty gush of a spoonful of water, suddenly stopped with excessive pain) were more
I i i frequent,

frequent, and were attended with a *tenesmus* and irritation at the end of my yard. Mr Ranby the surgeon, and Mr Graham the apothecary, having often visited me, and having got constant accounts of my disorder, and the symptoms that accompanied it, both declared, there must be a stone in my bladder. I was willing to be probed; but, as I had no thoughts of being cut, Mr Ranby declined undertaking that troublesome office, being persuaded, without the trial, I had a stone in my bladder. Lord Barrington, hearing of my complaint, was so good as to send me the volume of Scotch Medical Essays, containing Dr Whytt's account of the good effects which taking soap and lime-water had had in cases similar to mine, with ingenious reflections and directions relating to that cruel disease, and the remedy for it. I read them with great satisfaction, and would have immediately fallen into that method; but my relations, touched with the fatal effect which Dr Jurin's *lixivium* had had upon the late Lord Orford, would not suffer me to follow my own inclinations.

WHILE I had a severe fit upon me, I was visited by the Earl of Morton, who, upon hearing what was my disorder, gave me an account of the powerful benefits and entire cure which Mr Somers* had found,

* Late one of the commissioners of his Majesty's customs in Scotland.

The Earl of Morton having, at this time, wrote to Mr Somers to know more particularly the method of cure which he used, received from him the following letter, which his Lordship was pleased to give me, with allowance to publish it with Mr Walpole's case.

My LORD,

" In obedience to your Lordship's commands, which reached me yesterday, I have the honour to send you an account of the regimen which recovered me from a most deplorable illness that had, a long time, baffled our ablest physicians.

" I took four drams of Alicant soap, four times a-day, early in the morning, at eleven before noon, at five after noon, and at going to bed. Upon a distaste I have to pills, I dissolved each dose in about half a pint of oyster-shell lime-water. To make the draught more palatable, I added a little milk. The lime-water, thus mixed, was the only liquid I drank during my indisposition; and, as I think its power alone sufficient to prevent any new concretion, I intended to continue in that practice. By this method, my dismal complaints, in a few weeks, vanished; and, in about two months, I happily voided a small stone, quite smooth,

found, in voiding the stone that had tormented him for many years, by adding lime-water to the soap, which he had taken for some time without success.

THIS example, by the encouragement of Mr Graham my apothecary, fixed my resolution to follow that method; and accordingly, before I left the town, I often perused Dr Whytt's essay relating to the stone.

IN March 1747, I began at first with taking every day half an ounce of Alicant soap, made into pills, with a syrup of marshmallows, and drank upon it about a pint of lime-water made of oyster-shells, mixing a spoonful of milk with it, and drinking a spoonful after it, to take away the nauseousness of the taste.

UPON the road, as I went into the country, in May 1748, I had a most severe fit at Newport, making bloody water, with frequent interruptions, and short intervals, attended with violent pains, which continued upon me to such a degree, that I could not endure the horses to go more than a foot pace for above seventy miles, till I got home.

AFTER my arrival there, I was tolerably well for some days; but the least motion in a coach, or even in walking, brought the disorder upon me. I was always (which is remarkable) intirely easy when I lay a-bed; but was obliged, when I got up, to take to my couch, and could not venture to move from thence but on some necessary occasion. In the mean time I continued to take the soap and lime-water, which, by degrees, I increased so far as to take, at different times, an ounce of soap, and three pints of lime-water a-day; observing a very regular diet. After some months I found myself extremely easy in my ordinary motions; but I never ventured to walk far, nor go at all in a wheel-carriage, keeping my-

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self

“smooth, and to be sure much diminished by this dissolvent; having frequently before discharged gravel of the same colour. I heartily wish it may have the same effect upon your Lordship's friend.”—I am, &c.

EDINBURGH, November 10.

1747.

self as quiet as I could, until I should be obliged to go to parliament.

Just before I left the country, Mr Ranby made me a visit; and, altho' I had felt no pain or symptom of my disease for some time, he advised me not to hazard going to town, by any means, unless in a litter: however, having caused a voiture to be made, I undertook the journey in it, the 20th December 1748, which was regulated by the horses going no faster than a gentle walk, and but twenty miles a day.

THE cold weather, and the tediousness of creeping so slow, made the coachman sometimes fall into a trot; which I perceived; but finding no inconvenience, did not check his pace. The set stages were observed; but the last two days, and particularly the last day, the coachman drove from Harlow to White-chapel, as full a trot as the horses could go at any time, and I felt not the least disorder. I took a chair at Whitechapel, and all that winter used nothing else, and continued extremely well: But, about two months after my arrival in town, I found some small uneasiness in making water, and in two or three days, I voided, with my urine, something of a flat shape, about the bigness of a silver penny, covered with a soft white *mucus*, which, when it was dry, was plainly of a stony substance, and, after that, have never since been troubled with the least symptom of that cruel disease. And I found myself so well in the country, last year 1749, that, contrary to the advice of all my friends, I undertook, in my coach, a journey to Chatsworth in Derbyshire, at least 160 miles from my own house in the country, to pay a visit to the Duke of Devonshire, the horses going as round a trot as they could conveniently according to the road; and the last 10, or rather 15 miles, from Hardwick to Chatsworth, a most rugged and rocky way, we neither spared ourselves nor our horses; and although the great shocks upon the stones broke the springs of my coach, yet they gave me not the least uneasiness; and I have ever since continued, with respect to my former disorder, as well as I ever was in my life. But I now and then voided some red gravel, after I had sat a great while in the house of commons.

As

As I never perceived that I voided, during my illness, any fleaks of a stone, besides the above-mentioned, and was never searched by any instrument; I can no otherwise pronounce it to be a stone, unless by the symptoms I felt, and the judgment of the surgeon and apothecary that attended me, from these symptoms.

BUT it is very remarkable, as I have said before, that I never felt these symptoms while I lay a-bed, nor to so great a degree when on my couch as upon my legs; which looks as if the posture made great alteration; and that, methinks, could not have been the case, if I had been troubled with a scorbutic corrosive humour only. I must leave it to the learned in physic, to make what conclusions they think fit from this true state of my case. I think I remember in some of Dr Whytt's observations, that although the soap and lime-water were not able to dissolve or bring away the stone, yet they might cure its painful symptoms, and hinder it from vulnerating any part of the bladder, by blunting its sharp points, rendering its surface smoother, and even covering it, in some measure, with a kind of mucilage. This may possibly be my case, if I have still a stone there; and therefore I continue to take the third part of the soap and lime-water daily, which I used when I took the full quantity.

April 21. 1750.

H. WALPOLE.

The SEQUEL of Mr WALPOLE's Case.

Containing an account of the state of his health, with respect to the stone in his bladder, from November 1750, to the end of April 1752.

Cockpit, April 28. 1752.

AFTER having found myself, for two years together, perfectly well, and free from all symptoms of my former disorder, I took no more than one third of the soap and lime-water that I had formerly used.

IN November 1750, I came out of the country in my coach, in the usual travelling pace, without the least inconvenience; but having ventured, after I came to town, to go now and then in a coach upon the stones, I began, at times, to feel the symptoms of my former disorder, which, upon any motion, besides that of going in a chair, even by walking to any degree, increased upon me; and driving only in my chariot, through the parks, to Kensington, without going upon the stones, I found myself much troubled with making frequently and involuntarily, water, sometimes bloody, though not with much pain.

HOWEVER, taking the precaution of going by water, as far as the Old Swan, and being carried from thence in a chair as far as White-chapel, I ventured in a chariot, fitted up with the best French springs, to go into the country with Mrs Walpole, last June about mid-summer; but before I had got half way to Epping, tho' the horses went but a slow pace, I felt as great uneasiness, attended with the same severe symptoms, as I had ever done; which frequently returned and continued upon me, during the whole journey, for four days together, with little or no abatement, but while I was in bed; where, as formerly, after I had lain some time, I was perfectly easy the whole night.

As soon as I got out of my chariot, upon my arrival at my house in the country, I had indeed a cruel fit; but after I had rested one
night,

night, and kept myself as quiet as possible, for a few days; I found myself perfectly well again: and as I never went in a coach, and did not walk much during my whole stay in the country last year, for about five months together, I never felt the least symptom of uneasiness.

SOME few days before I left the country, I took a turn or two round my park, in my chariot, free from pain, which encouraged me to undertake a journey to town again, last November, in my chariot, by short stages and gentle driving; and it was performed, in five days, to White-chappel *, without being sensible of the least inconvenience any part of the way: Neither have I felt any since my arrival in town, and I still continue well, taking daily, as I have constantly done since June 1751, when I went last into the country, the full quantity of soap and lime-water that I formerly took, viz. an ounce of the former, and near three pints of the latter.

H. WALPOLE.

MR WALPOLE continued well from April 1752 to September 1754, taking about an ounce of soap, and three pints of lime-water daily; but finding, of late, that the soap opened his belly too much, he has since taken it in less quantity. In summer 1754, he frequently rode in his coach forty English miles a-day without the least uneasiness, or ill consequence: and he says he is now quite well, and rather in better health than formerly, believing that lime-water is a great cleanser and sweetener of the blood.

* From Mr Walpole's house in Norfolk to London, is about a hundred miles.

II. A further continuation of the CASE of the RIGHT HONOURABLE HORACE Lord WALPOLE, with an account of what was observed after his death, upon opening his body; being the copy of a letter from John Pringle, M. D. F. R. S. to Dr Robert Whytt professor of medicine in the university of Edinburgh.

• S I R, †

London, 22d Feb. 1757.

I IMAGINED, that upon hearing of Lord Walpole's death, you would be desirous to know the state of his Lordship's health from the time he published his own case; whether he continued the use of his medicines to the last; what distemper he died of; and, if his body was opened, what was the condition of his bladder and kidneys. I informed myself, as well as I could, of all these particulars; and I hope I shall be able to give you some satisfactory account of most of them.

LAST year, in the month of March, about ten months before his Lordship's death, I happened to meet him at a friend's house, where he dined, and never saw any man of his age with a more healthful appearance. He was then in his 78th year. He eat with an appetite, and of a variety of dishes; drank some Madeira, and was very chearful the whole time. His Lordship then told me, that he had enjoyed perfect health since he sent his case to the Royal society; that he thought it probable there was still a stone in his bladder, but so diminished, or smoothed, as to give him no uneasiness; that he did not think it safe to go about the streets of London in a coach, but that he went every where in a chair; and that, in the country, he could travel 40 miles a day in his post-chaise, without fatigue, or feeling any of his old pains upon the motion. That he continued to drink, for a constancy,
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* Read April 21. 1757.

† Vid. Philosophical Transactions, vol. 50. part 1. p. 205.

three pints of oyster-shell lime-water daily; and to take, as often, from half an ounce to a whole ounce of soap, by way of lenitive. All these circumstances I am sure of, because I noted them down when I came home.

FROM this time to the beginning of winter, Lord Walpole (as Mr Graham, his apothecary, informed me) continued in the same state of health. But some time after coming to town, his Lordship was seized with a lingering feverish disorder, very much affecting his spirits, but entirely unconnected with the stone. Dr Shaw, who attended his Lordship for about a fortnight before his death, told me, that there had never been any stoppage of water or passing of bloody urine, or any pain about his bladder or kidneys, during his last illness; but that he now and then felt some irritation in making water, a symptom too inconsiderable to require any other medicine than the continuation of his lime-water: which, in a smaller quantity, he drank till within two or three days of his end.

MR RANBY and Mr Hawkins, surgeons, with Mr Graham, were present at the opening of the body; and from the two last I received the account of the dissection.

THE coats of the bladder appeared to be a little thicker than natural, but were otherwise found. The *glandula prostatica* was of a large size, but distempered. They found three *calculi*, two lying loose in the bladder, and the other, a very small one, sticking in the passage, at that part which is surrounded by the prostate gland. Mr Graham favoured me with a sight of them all. The two first were very much alike, being of the shape and size of the kernel of a Spanish nut; only the sides were irregularly flattened, but without forming any sharp angle. The surface of each was everywhere smooth, except where there had been a separation of some small scales, not so thick as one's nail; and the largest exfoliation from one of these stones appeared to have been nearly about the breadth of the nail of my little finger. The polish otherwise, as well as the colour of both, might be compared to a boy's marble. One of these *calculi* weighed 21 grains, the other 22 grains: they were heavy for their bulk, and seemingly of a hard substance.

The smallest stone having been put up with some others of the same size, taken out of the gall-bladder, Mr Graham could not be positive which of them it was; and therefore I can only say, that what he thought most likely to be so, was about the size and shape of the seed of an apple, with the point broken off and the edge ragged. This, as I observed, was found in the passage, seemed to be coming away, and probably had occasioned that irritation the patient had now and then felt during his last illness. It weighed only about a grain.

No parts could have a sounder appearance than both the *ureters* and kidneys. The first were not dilated; nor did the last contain any stone, *mucus*, or gravel: the *pelvis* in each was of a natural size.

THE rest of the abdominal *viscera* were in the same healthful state, except the gall-bladder, which was full of stones. The largest was about the size of a small chestnut, but rounder. The surface was smooth, particularly at one part, where it seemed to have rubbed upon a lesser *calculus*, of the shape of one of the *vertebræ* of a small animal, without the processes. This last had a hollow on each side corresponding to the convexity of the large stone; and these cavities being finely polished, it seemed as if sometimes one side, sometimes the other, of the small stone had been turned to the great one, and had been shaped in that manner by the attrition. The largest *calculus* weighed one drachm two scruples and two grains; the small one but nine grains: they both sunk in water; and felt specifically heavier than any stones I have ever seen taken out of the gall-bladder. Besides these two, there were several very small *calculi* of irregular shapes, and of rough surfaces, which all together did not weigh above five grains. Mr Graham, who had attended his Lordship for about 40 years, assured me, that he never had any symptom that indicated a stoppage of the bile, or the passage of a stone from the gall-bladder into the intestines.

NEITHER the head nor breast were opened.

THESE

THESE are all the materials I can furnish you with, relating to this case. If you desire to be more particularly informed of any of these circumstances, let me know, and I will endeavour to procure you all the lights I can. In the mean while, I should be glad to have your remarks upon what I have now sent you; and since you have been so long in the train of thinking, with more than usual attention, on this subject, I presume it would be very agreeable to the gentlemen of the Royal Society to have a paper from you on this occasion; and the rather, as his Lordship began his course of soap and lime-water, upon hearing of your success by that method of cure. I am,

SIR, &c.

JOHN PRINGLE.

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III. Some

III. Some OBSERVATIONS on the CASE of the late RIGHT
HONOURABLE Lord WALPOLE, of Wooltertoon: In a
letter to Dr John Pringle, F. R. S.

Edinburgh, March 16. 1757.

* SIR, †

PHYSICIANS have not, perhaps, differed more widely in any thing than in their opinions of the medicines lately proposed for the cure of the stone. While some imagined, that Mrs Stephens's medicines, or soap and lime-water, were in most cases to accomplish a dissolution of the stone; others have been positive, that nothing of this kind was to be expected from them: nay, they have condemned these medicines, when used in large quantities, and long persisted in, as hurtful to the stomach, guts, and urinary passages; and have ascribed the remarkable ease which they almost always give to calculous patients, to their depositing a calcarious powder upon the surface of the stone, by which it is rendered less hurtful to the bladder. And this opinion seems to have been not a little strengthened, by the great quantity of white sediment observed in the urine of those patients who have used soap and lime-water in considerable quantities. Now, as I am of opinion, that most of these objections and doubts, concerning the effects of soap and lime-water in the cure of the stone, may be cleared by a candid consideration of Lord Walpole's case, I shall trouble you with a few remarks, which have occurred to me, in comparing it with the appearances found in his Lordship's body after death, of which you were so obliging as to send me a particular account.

I. WHATEVER doubts may have been entertained concerning the cause of Lord Walpole's complaints, yet it now appears evidently beyond dispute, that they must have been owing, not to a
scorbutic

* Read April 21. 1757.

† Vid. Philosophical Transactions, vol. 50. part 1. p. 209.

scorbutic corrosive humour in his bladder, as was imagined by some, but to stones lodged in it. These stones may possibly have lain there since 1734; for from that time to spring 1747, his Lordship was free of any gravelish complaints, only passing some red sand at times. But at what time soever they may have first arrived in the bladder, in 1747 and 1748, they seem to have acquired such a bulk, or were become so rough or pointed in their surface, as to occasion great pain, frequent provocations to urine, and sometimes bloody urine; especially after any considerable motion. These complaints, however, were soon relieved, by swallowing daily an ounce of Alicant soap, and three English pints of lime-water made with calcined oyster-shells: and from 1748 to 1757 his Lordship was kept almost intirely free from any return of them, except for some months of 1750 and 1751, during which he took only one third part of the quantity of soap and lime-water above mentioned.

2. IT is highly probable, nay, I think, altogether certain, that the soap and lime-water not only relieved Lord Walpole of the painful symptoms occasioned by the stones in his bladder, but also prevented their increase.

IF these stones came into the bladder in 1734, they must, in so many years as his Lordship lived after this, have acquired a very great bulk: nay, if we suppose them not to have been lodged in the bladder above a year before they began to occasion frequent inclination to make urine, with pain, and sometimes sudden stoppages of urine; yet, from 1746 to 1757, they ought to have grown to a much larger size than that of the kernel of a Spanish nut *. It is true, the stone may increase faster in some patients, and slower in others; but stones, after remaining a dozen or more years in the bladder, generally weigh several ounces. Some years since I saw a stone, weighing near six ounces, taken from a boy of no more than 14 years of age.

3. LORD

* The stones found in Lord Walpole's bladder were of this size, and weighed one of them 32 and the other 21 grains.

3. LORD WALPOLE's case not only shews the power of soap and lime-water to relieve the painful symptoms, and prevent the increase of the stone in the bladder, but also makes it probable, that these medicines do communicate to the urine a power of dissolving the stone.

IN the beginning of 1749 his Lordship voided with his urine a calculous substance of a flat shape, about the bigness of a silver penny, and covered with a soft white *mucus*; and upon the surfaces of the stones found in his bladder there were some inequalities, which seemed to have been made by the separation of thin *lamellæ* or scales. Further, the small stone found in the beginning of the *urethra* must have been in a dissolving state, and considerably lessened in the bulk: for, if it had lain long in the bladder, and never been larger, it ought to have been voided thro' the *urethra* with the urine; and it could not have arrived lately in the bladder, since Lord Walpole had not had, for several years before his death, any nephritic pains, or symptoms of stones passing from the kidneys; and since it is not likely, that a stone of the size and shape of the seed of an apple * would pass thro' the *ureters* without being felt. Now if this small stone, found in the *urethra*, was partly dissolved by the virtue of the soap and lime-water, it will appear at least probable, that the two larger stones in the bladder were so likewise. But although Lord Walpole's calculous concretions had remained undiminished, and without any symptoms of dissolution, it would not therefore follow, that soap and lime-water cannot dissolve the stone in other patients, where the concretion may be of a less firm texture.

THE Rev. Dr Richard Newcome, now Lord Bishop of Llandaffe, while drinking two English quarts of lime-water daily, for the cure of the stone in his bladder, poured his urine every morning and evening upon a piece of human *calculus* weighing 31 grains; by which, in the space of four months, it was reduced to three pieces, weighing in all only six grains. Upon one of these pieces, weighing 2. 31 grains, he caused to be daily poured, for two months, the
fresh

* The stone found in the beginning of the passage from the bladder was of this size, and weighed about a grain.

fresh urine of a person who drank no lime-water; at the end of which time the piece of *calculus* was found to weigh 2.56 grains, having increased in weight a quarter of a grain. This same piece being afterwards steeped in the Bishop's urine, (who continued to drink lime-water as above), from June 24. to July 9. was in these few days quite crumbled into powder. Since this experiment shews, beyond dispute, that lime-water, unassisted by soap, can communicate to the urine a power of dissolving the stone out of the body, it can scarcely be doubted, that it must have the like effect on it when lodged in the bladder. And that the dissolution of the stone in the bladder has been completed by soap alone, appeared evidently in the case of the Rev. Mr Matthew Simson, minister of Pancaitland near Edinburgh; an account of which will soon be made public by Dr Austin, who opened his body after death. Mr Simson had, from 1730, been afflicted in a less or greater degree with the symptoms of a stone in the bladder; and in November 1735 was sounded by Dr Drummond of Perth, and Mr Balderston, surgeon in Edinburgh, by whom a stone was not only plainly felt, but also by the patient himself. In February 1737 he began to take soap; and after 1743 never had any gravelish symptoms. He died in May 1756; and, when his bladder was looked into, there was neither stone nor gravel found in it.

4. It appears from Lord Walpole's case, that soap and lime-water, even when taken in large quantities, proceed very slowly in dissolving the stone.

FROM July 1748, to the beginning of 1757, his Lordship drank three English pints of lime-water, and swallowed for the most part an ounce of soap, daily; except from April 1750 to June 1751, during which time he took only one pint of lime-water, and one third part of an ounce of soap, daily. However speedily soap and lime-water may dissolve the greatest part of urinary stones out of the body, yet being mixed with the aliment and humours of the stomach and guts, and afterwards with the whole mass of blood, it is impossible but their force must be greatly impaired before they arrive

arrive with the urine at the bladder. When, therefore, urinary stones are of an uncommon hard texture, we are perhaps scarcely to expect any sensible dissolution of them by the use of soap and lime-water: but when they are of a softer kind, there is no reason to doubt, that these medicines will in time dissolve them; and this will happen sooner or latter, in proportion to the hardness of the stone, to the quantity of the medicines swallowed by the patient, and the exact regimen he observes as to his diet.

BUT, however slowly soap and lime-water may proceed in dissolving the stone, yet they generally give speedy relief to the patient. Lord Walpole did not take these medicines in the full quantity till the end of July 1748; and, in a few months after, he was not only greatly relieved of all his complaints, but in December was able to ride an hundred miles in his coach, without finding any uneasiness, altho' the two last days of the journey the horses went at a full trot. In winter 1750, and spring 1751, when his Lordship swallowed only one third part of the soap and lime-water which he had been in use to take, his pains and frequent inclination to make urine returned in a good degree; but, after taking the medicines in the full quantity, he soon became as easy as before.

IT would seem, while Lord Walpole used only one pint of lime-water and one third of an ounce of soap daily, that the petrifying quality of his urine was not intirely destroyed, and that the stony particles newly formed on the surface of the *calculi* occasioned, by their roughness, the return of his painful symptoms. However, when he had recourse to the medicines in a larger quantity, the petrescent quality of his urine was not only destroyed, but this fluid seems to have acquired a power of dissolving the rough stony particles deposited on the surface of the *calculi*; and in this way soon removed the pain, bloody urine, and frequent desire to make water, upon using any considerable exercise.

SOAP and lime-water not only relieve the painful symptoms occasioned by the stone, by wearing off its sharp points, and rougher parts, which used to irritate the tender membrane which lines the bladder; but, when this membrane has been wounded or lacerated by

by the stone, there is nothing that will heal it more speedily than lime-water; which the ingenious Dr Langrish has found to be remarkable also for its effects in curing the bladders of dogs, after being fretted with soap-leys *.

THE power of soap and lime-water to alleviate the painful symptoms attending the stone is so great, that, as far as I remember, I have only met with one patient, who did not find himself considerably relieved by them. But it is to be observed, that this patient neither took them in full quantity, nor persisted in their use for a long enough time: and, when he was afterwards cut, the stone taken out of his bladder was almost as thick set with sharp prickles as the back of an hedgehog: so that, in this case, no remarkable ease could be procured to the patient by the medicines, until they had quite dissolved these sharp points, and rendered the surface of the stone smooth and equal; which was not to be done but after a very long time, especially as the stone was of a pretty hard texture.

IT may be proper to take notice, that when, along with the stone, there is any ulceration in the bladder, soap does mischief, and lime-water often fails of giving any considerable relief. However, even in this case, it is perhaps one of the best remedies we know.

5. SOAP and lime-water, taken in large quantities, and persisted in for many years together, appear to be innocent, and no way injurious to health.

LORD WALPOLE, who used these medicines for upwards of eight years, was not only relieved of the painful symptoms of the stone, but had his health improved by them in other respects. His appetite, healthful look, and a degree of spirits uncommon at his age, continued till the end of 1756, when his last illness began first to attack him. And as his health did not appear to be any way injured by these medicines; so, when his body was opened after death, his kidneys and ureters were observed to be quite sound and natural, as was likewise his bladder; only its coats appeared a little

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* Physical Experiments, p. 19.

thicker than usual, owing probably to the long-continued friction of the stones upon it. Neither the kidneys, ureters, nor bladder, were loaded or crusted with any calcarious matter; an effect most unjustly ascribed to soap and lime-water, since in the urinary passages, to which the air has no access, they cannot deposit their calcarious part *; and since the white stuff observable in the urine of such patients as take these medicines in large quantities is only the usual sediment of the urine changed in its nature and colour, with, perhaps, some of the dissolved particles of the stone †.

As the urinary passages were no-ways injured, so neither were the stomach, guts, and other *viscera* of the lower belly. These had all a healthful appearance, except the gall-bladder, which was almost full of biliary concretions: nor is it surprising, that soap and lime-water, which prevent the growth of urinary *calculi*, should have no effect on biliary stones, since, although these medicines dissolve the former out of the body, yet they do not make the smallest impression on the latter.

I presume it will be needless to take notice, that the lingering nervous fever, of which Lord Walpole died, cannot, with any colour of reason be ascribed to the large use of soap and lime-water; since, if they could have produced such an effect, they must have done it in much less time than eight years and an half.

It may not be amiss to observe, that although soap and lime-water, taken in large quantities, are noways injurious to health, yet in some cases they may become improper, on account of the particular state of the patient. Thus, in a scorbutic or putrid disposition of the humours, soap at least ought to be totally omitted; and such patients, who are much troubled with the hæmorrhoids, ought to be sparing in its use, as the alkaline salt, with which it abounds, will scarcely fail to exasperate their pain. Where the patient is naturally very costive, less lime-water and more soap ought to

* Essay on lime-water, p. 421.

† Essay on lime-water, p. 349. &c.

to be used; and, on the contrary, where the body is too loose, little or no soap is to be taken, but the cure is to be trusted to lime-water alone; which, in this case, ought to be drank to the quantity of two English quarts a-day.

As the foregoing observations will, I am afraid, appear more tedious than important, I shall only add, that I am, with great esteem,

S I R,

Your most obedient humble servant,

ROBERT WHYTT.

IV. CASE of the Reverend Dr RICHARD NEWCOME
Canon of Windsor, afterward Lord Bishop of Llandaffe.

Written by himself.

THE Reverend Dr Newcome, Canon of Windsor, had for a twelvemonth voided large, round, red gravel, without any pain or discoloured water. In April 1751, after riding, his water first appeared bloody, which was attended with a frequent irritation to make it, tho' with very little pain. After one day's rest, the water would return to its natural colour, and he was able to walk about, or use exercise, without any inconvenience, for nine or ten days; in which time his symptoms usually returned; but soon went off again upon his sitting still. These intermissions of his disorder grew gradually less, and a less motion would occasion uneasiness and bloody water; so that in the November following he had hardly any intermissions at all, and could scarce stir from his chair without being obliged to pass his water immediately; and with any little motion, was sure to void blood. When his uneasy sensations had been quieted by sitting still, they would constantly return, if he only walked across his room. In this state, he began to drink lime-water, and the following account of its success is in his own words.

I began to take the lime-water Nov. 1751, and in a few days brought myself to drink two quarts a-day, taking no other liquid whatever, except tea; and this I have continued with very little interruption for above two years. I found no immediate effect from it, but continued all the winter making bloody slimy water, with irritating pain upon every little motion, and a constant inclination to go to stool; but, by keeping quiet, I continued pretty easy. The greatest exercise I took, was walking to church (not half a quarter of a mile distant) and officiating every Sunday; and by this I never failed having my urine discoloured, and the irritating pains more or less: yet I began about February to think myself better, at least

I was satisfied that I was not grown worse; and being obliged, by bad weather, to go to church in a chariot, tho' I felt uneasiness, I thought I bore it better than I had formerly done; I began too to be less disordered by officiating at church, but still always in some degree, till Easter-Sunday, when, being obliged to stand much longer, and move more, I expected the usual consequences of blood and pain; but neither happened, nor have ever since, as they used to do by the same exercise.

I was certain now I could bear exercise better than I had done; and, encouraged by this, I ventured to ride on horseback, which I bore once very well; but, on a second trial, had the usual consequences of blood and pain; and about a fortnight after, in the month of May, thinking myself abler, I tried the motion of a chariot; but, tho' driven very slow, I could not bear it half a mile, and was forced to get out and walk home with difficulty, making much blood with great irritation. From this time to the latter end of June, I kept quiet and was easy, and, I concluded, better than before I began the lime-water; but setting out on a journey to London in a coach, I suffered much in the journey, and had all my former complaints in a greater degree than ever. A week or ten days rest, after my journey, recovered me to my former ease; but I could bear but little motion.

I now began to take soap with the lime-water: in the middle of July, I was obliged to go to Windsor; which journey brought on my usual complaints, and I continued the three weeks I was there very indifferent, having continual uneasiness, gripings, dregs of blood, irritations, and a constant feel of going to stool, and passing, at the same time, a little black sand; but nevertheless I bore the motion of the coach in my journey back so much better than I ever had done before, since I had been ill, that I could not but conclude I was much better.

AUGUST 1752, I was searched, and a stone found in my bladder. I continued in town till the beginning of October, growing better, as I judged, by my being able to move more without bringing on pain and blood; and I was confirmed in my opinion, by bearing a journey

journey of 160 miles to my own home with but little blood or pain, in comparison with what I had when I came up three months before.

ALL this winter I kept quiet and enjoyed perfect health : And by February 1753, the irritations and frequent motions to make water and go to stool had left me, and my urine was without slime and a sort of oiliness it used to have, and from a dirty colour, was become yellow ; but at times there would be upon it a strong scum or froth which reflected colours like mother-of-pearl, and would remain on paper if not touched (but it could not resist the least touch) a day or two, till it dried to a fine powder. I have not observed any of this scum these last six months, nor, as far as I can judge, since I left off soap.

I had all this while carefully observed my urine, but could not discover any fragments of stone, nor had ever the least feel in the passages, as if any thing had made its way. But, about March 1753, in a pot that had been emptied, and by accident not washed, I discovered hanging, about the sides, white particles like mortar, which seemed to be a stony substance, and some few of them I could plainly perceive to be convex ; but I observed very few of this sort, and indeed not many of any kind at a time, although very constant ; and it is probable that I might have passed them long before I attended to it.

THE state of ease I found myself in at this time (February 1753), I had come into gradually, and I thought it might in great measure be owing to my keeping quiet, which I determined to continue ; but undesignedly got the habit of stirring more about ; and found I had little or none of my former complaints, which had left me so gradually, that I cannot say when it was I was totally free from them. In March the bad weather again obliged me to use a chariot to church, and I felt not the least uneasiness from the motion. I now began to walk about without restraint two or three miles at a time, and went airing in the chariot upon paved roads, without any inconvenience, though driven full trot.

IN

IN July 1753 I undertook a journey to London, and bore the coach as well as I ever did, and in town went several long and rough stages, in a hackney-coach, on the stones, and from thence to Windsor and back again, and then down again to Whitechurch; and all this without the least uneasiness whatever. But, about this time, I began to pass some red gravel without pain, which I had not done before for two years, and at my return home in September, had a great pain in my loins, which moved at last into the groin, and for a fortnight or three weeks some gravel passed every day, but without discolouring the water, or any irritations, or any of the former sensations. I have from this time been subject to pass gravel at times, but with no pain at all; and a week before I set out on my last journey to London March 1. 1754, some passed every day, which continued on the road, and does still much in the same manner it had done for a year before I complained of the stone; but I feel no pain, and can bear all sorts of motion as well as ever I could in my life.

FROM November 1751 to January 1754, I punctually took the full quantity of 2 quarts a-day of lime-water, making it my only liquor, except tea, and about half the time an ounce of soap a-day, and never found myself better with regard to appetite, sleep, and indeed in all respects. And the lime-water which I took at first with some reluctance, not only came by use to be tolerable to me, but as agreeable as any other liquor.

SEPTEMBER 1753, I began to pour, morning and evening, some of my urine on a piece of human *calculus*; in about two months its surface began to turn white, and to appear softened; and, upon touch, retained the impression of the skin of the finger; but this, only just on the outside, without any depth; and the stone remained hard and firm as before. By degrees the surface grew more soft, and seemed loose and hollow from the body of the stone, as a shell to it, and, in a few days, began to crack (in November) and scale off: I then took it in my hand, and found it very rotten, so as not to be touched without breaking bits from this outward shell, under which the stone was still hard, tho' honey-comb'd; although the
surface

surface was so rotten, as not to endure the slightest touch, without crumbling into coarse mortar; yet, if let alone, little came off in bits; but it wasted imperceptibly. As the outward surface came off, there appeared a small crack round the stone, as if marked out with the point of a knife; this daily grew wider and deeper, and, upon examining the stone in my hand, it came in two in that place; and the surfaces, by which the two bits had adhered to each other, were quite smooth, and not incorporated into each other, so as to form one mass, but as if they had been originally two distinct bodies stuck together by a cement.

IN December another layer came off in the same manner, upon only turning it out of the bottle; but I believe neither of these would have come off by themselves; for there seemed to be other *strata* separating in the same manner, which being let alone, never came off in bits, but went off in a sort of mucous slime. As the stone was now in three pieces, and those grown thin, it began to waste very fast, and I was willing to save them; and therefore discontinued the experiment in the beginning of January, just four months after I began it. [In which time this bit of *calculus*, which weighed at first thirty-one grains, was reduced to six grains, and these so rotten as hardly to hold together.]

IT is to be observed, that I took no soap during the time of this experiment. The urine was gradually made into the phial, so as not to fall with any force on the stone, and care was taken all the while, that no shock should be given to the stone, by shaking the phial, or by any other means. The urine was changed generally three times in the 24 hours, but always twice.

ADDITION

ADDITION to the case of the Right Rev. Dr NEWCOME, Lord Bishop of Llandaffe.

ONE of the three pieces, into which the remainder of this bit of *calculus* was broken, was steeped in the same manner during the space of a month in the urine of another person who did not drink lime-water, without any sensible increase or diminution of its size or weight. The same piece, weighing gr. 2. 31, had the fresh urine of a third person, who did likewise not drink lime-water, poured upon it once at least every day for two months; at the end of which being taken out, washed in clean water, and well dried, it was become redder, and seemingly less porous, and was found to weigh gr. 2. 56, having increased in weight a quarter of a grain. The very same piece was again delivered to Dr Newcome, (who was still drinking lime-water), and was steeped in his urine, as before, from June 24th to July 9. and in these few days it all crumbled into powder.

It should be observed, that his Lordship still continues to void gravel, though his urine is kept so impregnated with the virtue of the lime-water, as to dissolve the bit of *calculus* immersed in it. This gravel seems to be, what is formed in the kidneys; where consequently the lime-water has no power: Though it will hinder the gravel when fallen down into the bladder, from uniting or growing into a stone, by dissolving it there, if it should not happen to pass with the urine.

V. CASE OF MR YOUNG GREEN.

[Communicated by Mr Alexander Campbell surgeon at Pool.]

MR Green was, from his twelfth year, subject to frequent sudden obstructions in making water. In his 15th, on riding a hard-trotting horse, he felt violent pains both before and after the discharge of his urine, attended with a *tenesmus*. The symptoms increasing by degrees, the least extraordinary motion brought on a paroxysm, during which he sometimes voided bloody urine: A sensation of great weight and pressure about the *anus* was now his constant complaint, except when in bed. Sometimes his urine flowing in a full stream, suddenly stopped, and came away by drops, and he felt the stone rolling gradually back in his bladder, as its contraction and the endeavour to urine diminished. Though I apprehend these symptoms to be the almost infallible diagnostics of the stone in the bladder, yet a learned physician, who was consulted at this time, gave it as his opinion, that the disorder was an inflammation in the neck of the bladder.

ALTHOUGH Mr Green wished the Doctor's opinion to be well founded, yet his strict adherence to the soap and lime-water was a manifest proof that he believed the contrary. He continued the use of both, from April 1751 to the month of September following, with little or no interruption; during which time, the great weight and pressure about the beginning of the *urethra* gradually abated.

HE frequently now made water without pain; and when he had any returns of it, it was neither severe nor lasting, but of a sharp smarting nature: In short, he owned more than once, that he would gladly continue the use of the medicines during the remainder of life, could he be assured of enjoying the same degree of ease he then felt, without a prospect of better health.

His urine during this time let fall a copious sediment; sometimes

times, especially after a fit, a thick mucilaginous substance, which being dried, yielded 57 grains of a greyish powder, interspersed with shining particles, not unlike the inner *stratum* of oyster-shells. In September his business led him to London, where he was cut by Mr Sharp. Ten days after the operation he died.

THE stone is of a dark brown colour (but I was informed its surface was much lighter and in some parts white when first extracted) of the figure and shape of a mulberry with sharp starts sticking out around it, of a hard glassy texture. As it was the opinion of some, that these starts were formed from the particles of lime concreted on its surface, I with a pin cleared away the root of one of them, which I could trace deep into the substance of the soft brown matter of which the body of the stone consisted; a convincing proof that they were originally formed there; and I think it appears no less evident, that the body of the stone was wasted by the lime-water, while the harder shining points remained, in a great measure undiminished *.

* Did not the greyish powder, obtained by evaporating the sediment of the urine, consist chiefly of the softer part of the stone dissolved by that fluid; and were not the shining particles in it part of the sharp glassy starts, washed off, or, perhaps, partly dissolved by the urine?

M m m 2

VII. The

VII. The C A S E of WILLIAM HAY, Esq; • being an extract of a letter from Dr Ruffel of Brighthelmston, to Henry Pelham, Esq; communicated to Dr Whytt by the late Rev. Dr Stephen Hales, clerk of the closet to her Royal Highness the Princess of Wales.

November 25. 1755.

***** **M**R HAY took three ounces of Mrs Stephens's medicine in a solid form, every day for five years, never leaving off except for a few days at a time, to observe the effect of the medicine.

ABOUT five years ago he left off the use of these medicines, and afterwards pursued, with the same constancy, the use of Castile soap and lime-water mixed with milk. At first he took three ounces. About two years before his death he reduced the quantity to one ounce every day: under this method he grew so easy, that riding in a coach or walking seemed little to affect him. This inclined him to get on horseback, which he had not done before for eleven years. But he found an inconvenience from it the first time; and after the second time (which was a little before his last illness) he was heard to complain that his old distemper was like to return upon him again.

HE died apoplectic from a stoppage of the bleeding piles to which he had been accustomed for many years. He had neglected to use any evacuations; though they had been stopped for some months, and he was withal costive.

Two days after his death, Dr Ruffel was desired to attend the surgeon on taking the stone out of his bladder, which was empty of urine, and grown callous from its rubbing on the stone.

THE stone weighed three drachms, two scruples, and eight grains, was flat and oval, of a shining chesnut-colour, perfectly polished and smooth to the touch in every part. Being desirous to see what the
outward

• Author of Deformity an Essay, see above, p. 349.

outward *lamine* were composed of, I found the outer one thin and friable, the other thicker, and of a brown loam colour, and the stone to which they adhered, and was daubed over with, very rough.

• • • • •

“ FROM this short account of Mr Hay's case, it appears, that
 “ Mrs Stephens's medicines, or soap and lime-water, may give great
 “ relief to patients, and make them pass through life easily, even
 “ altho' they have little effect in dissolving the stone.”

VII. The

VII. The CASE of Mr L. TREVIGAR *. Written by himself, and communicated to DR WHYTT, by the late Mr Baron Edlin, November 1756.

IN the year 1748, I was seized with a most painful *sciatica*, for which I was ordered the warm bath, and this so weakened my nerves, that I soon fell into strong hysteric fits, with risings in the throat, swellings in the abdomen, tremblings, &c. I applied to Dr Ruffel, who, after having tried many other remedies in vain, advised me at last in the year 1750 to go to Brighthelmstone, and bathe in the sea and drink the water every day. This regimen I continued, without intermission, for six weeks, and, upon my return, drank the sea-water every other day. About a week after my return, I was seized with a violent pain in my kidneys, afterwards in my ureters, and at last, with the assistance of soft emollient medicines, brought away pieces of brown gravel of the size of a large corking pin's head, of the colour and hardness of brown sugar-candy. This was the first fit I ever had in my life; for tho' I had, for many years before, voided much red gravel, yet I never felt any pain till this time. But from this time I was frequently in great pain, especially after a little exercise on horseback or in a chaise, which constantly ended in a fit, and in my bringing away small stones as before. I advised with our Doctor and Apothecary for some years, and got some relief constantly from their oils, emulsions, &c.; but at last I was satisfied their oils, &c. only relaxed the parts for the stones easy passage, but never prevented their concretion. However, I continued them till the year 1752, when the paroxysms became so frequent and so violent, that scarce a fortnight passed without a most severe fit. I was then persuaded by Mr Hay, (who laboured under the same disorder, and had taken soap for seven years with great success, and without any the least inconvenience), to make use of his remedy; and accordingly in October 1752, upon my making coffee-coloured water, and a very cruel fit attending it, I took half an ounce of Alicant soap twice a-day, drinking after each dose half a pint of lime-water made of calcined oyster-shells. This regimen I continued without any inconvenience till

* Author of a treatise on Conic sections.

till May 1755, at which time I was taken ill with the hæmorrhoids to a very great degree; and as I was satisfied the symptoms of this disorder were greatly increased by the soap, I was obliged to discontinue it, and instead of drinking one pint of lime-water every day with the soap, I now drink, and have done ever since May 1755, three pints without it, a less quantity not being sufficient to keep me clear of the gravel. But by continuing this quantity of lime-water, even without any soap at all, I am perfectly easy, and have been so ever since May 1755; and so indeed have I been ever since I first took the soap, having never been troubled with one fit for these four years past, or had any symptoms of any stone's forming either in my kidneys or bladder.

As I have, in the foregoing case, mentioned Mr Hay, it may not perhaps be unseasonable to take notice, that this gentleman died about a year ago, and his body, according to his own request, was opened. I was not present myself at the dissection, but received an exact account of every particular from a gentleman of the faculty who was there, and attended with great accuracy to every part, expecting to find very extraordinary disorders occasioned by the immense quantity of soap taken. (I have heard Mr Hay himself say he had taken frequently three ounces in a day). But upon examination, no disorder was discovered that could be attributed to the soap. There was indeed a little extravasation in the *viscera*; but that, I am told, is no uncommon appearance, when people die of an apoplexy, as Mr Hay did; in all other respects he was perfectly found there. In his bladder was found a stone wrapt up with part of the coat on the left side, which formed a sort of purse for it, only the neck of the purse was open for the urine to pass into it. I saw and examined the stone myself; it was exceeding hard, but perfectly smooth, of the colour of a dark mohogany, and in the form of a convex lens, the distance between the two convexities being half an inch nearly. The breadth over was somewhat more than an inch and a quarter. There was not the least appearance of any corrosion, but probably the soap kept it smooth, and prevented its wounding the bladder, as it had before done; for from the time of his taking the soap, he was perfectly easy to his dying day.

VIII. The

VIII. The CASE of T. W. Written by himself in 1756.

IN June 1751, after having been for some years troubled with gravelish symptoms, I was attacked with a frequent inclination to make urine, attended with considerable pain. By the advice of a surgeon in the country to whom I applied, I used Spanish soap in small quantities for six months, without any advantage. On the contrary, I could neither ride nor walk without passing bloody water, and being affected with great pain; and when I slept hastily down stairs, or stumbled in walking, I always felt something weighty in my bladder. At this time, I was advised to have recourse to lithotomy; but I had such terrible apprehensions of this operation, that I could not think of submitting to it.

FROM November 1751 to November 1752, I was never altogether free of pain; but during the severe fits which frequently returned, and lasted usually two or three days, the pains were excessive, and I was obliged to attempt making water every three or four minutes.

ABOUT the middle of November 1752, after reading Dr Whytt's Essay on the virtues of lime-water in the cure of the stone, I began to follow the method of cure there laid down; and drank every day three English pints of stone lime-water, swallowing at the same time $1\frac{1}{2}$ ounce of soap. After the first week, my pains lessened somewhat, but I had no power to retain my urine for three weeks; on which account, I had recourse to oyster-shell lime-water, of which I drank the same quantity I had done of the stone lime-water, and continued the use of the soap. Within a few days after this, I began to be able to retain my water much longer, and had also less pain in passing it.

WHEN I had taken the oyster-shell lime-water and soap for six months, I found myself intirely free of all pain, and was able to walk some miles without any bad consequence. Soon after, I could ride on horseback, from four to sixteen Scotch miles without any pain.

I still continue the use of the lime-water and soap; and altho' I am in my 73d year, have no other complaint except that I am obliged to make water three or four times in the night; and after great fatigue, it is often a little tinged with blood.

IT

IT may be proper to mention, that, upon leaving off the soap for some time, I found my pains return in some degree, which obliged me to have recourse to it again.

“ THIS patient’s pains returning in some measure upon his discontinuing the soap, brings to my remembrance, that in making some experiments with different *calculi*, several years since, there was one almost as white as chalk, but of a less hard substance than the others; and which was not in the least degree dissolved or softened by being infused 20 days in oyster-shell lime-water, but yielded sensibly to a solution of Spanish soap in common water; for a piece of it weighing 30 grains, had near four grains of its substance dissolved in 14 days. From this experiment one may conclude, that it is better to prescribe both soap and lime-water for the stone, than any one of them alone; and that, if one of these remedies has failed of giving relief, the other ought to be tried; for as the above white *calculus*, which yielded to the solution of soap, resisted lime-water, so there may perhaps be others that are readily dissolved by lime-water, but little affected by soap.”

A N
E S S A Y

O N T H E

VARIOUS STRENGTH of DIFFERENT
LIME-WATERS.

First Published

In the Edinburgh ESSAYS PHYSICAL and LITERARY.

N n n 2

IN

ESSAYS

ON THE

VARIOUS STRENGTHS OF DIFFERENT
LIME-WATERS.

First Published

By the Edinburgh Medical and Physical Society.

1792

ESSAYS

ON THE

Various Strength of different LIME-WATERS.

THE Reverend and ingenious Dr Stephen Hales, having informed me, in a letter dated May 1751, that he had found the strength of lime-water much increased, by pouring it a second time on quick-lime, fresh from the fire; I thought it might be worth while to make a few experiments, in order to determine, with some degree of certainty, the different strength of different lime-waters; from these experiments it appeared, that lime-water acquired a considerable addition of strength by being poured on quick-lime newly taken from the fire; and that the first water got off quick-lime, was sensibly stronger than the fourth and succeeding ones *.

ON the other hand, my worthy friend and colleague Dr Alston, having observed, several years since, that quick-lime continued to communicate its virtues to water much longer than any one before had imagined; tells us, that he found afterwards, by experiments, that half a dram of stone quick-lime yielded forty ounces of lime-

* Essay on the virtues of lime-water, &c. p. 358.

lime-water; and that, after a pound of the same quick-lime had afforded five hundred pounds of lime-water, the water procured from it was as strong of the lime as ever *. Hence he imagines, that as water can only be impregnated to a certain degree, by quick-lime, so this will happen equally, whether the quick-lime be fresh from the fire, or has had five hundred times its weight of water poured on it before; provided the water be allowed time enough to extract the virtues of the lime †. And further affirms, that the strength of lime-water cannot be increased by slaking new-made lime in it, because the water can take up no more of the lime than it had before ‡.

As these experiments and conclusions appeared inconsistent with what I had advanced, the Doctor has endeavoured further to weaken the credit of my experiments, by some arguments drawn, chiefly, from the imperfection of the hydrostatical balance, and from the nature of quick-lime and its water. In order therefore to know whether I might not have been mistaken in what I had said concerning the strength of different lime-waters, I thought it necessary to make some new experiments; an account of which I beg leave to lay before the society.

I. (a) HAVING got from my ingenious friend Mr James Gray, a cylindrical copper-vessel ending in a narrow neck, which contained exactly 100 cubical inches; I filled it with the fountain-water of this city, and by means of a very nice balance, found it weighed 25320 Troy grains ||, besides the weight of the vessel itself, which amounted to 13055 grains.

(b) I poured upon 90 grains of calcined oyster-shells, newly taken from the fire, and reduced to a powder, 96 ounces, or five hundred and twelve times their weight of boiling water. After 92 hours,

* Philosoph. Transact. vol. 47. p. 266.; and Dissertation on quick-lime, &c. p. 4. 5. & 6. where the same thing is affirmed of oyster-shell lime.

† Dissert. on quick-lime, p. 11. & 53.

‡ Ibid. p. 11.

|| According to Mr Gray's experiments, the water which this vessel contains, only weighs 23518 grains, that is, two grains less than we have made it. This difference may

hours, during which time the infusion was frequently stirred and shaken, I decanted off the clear water, and filtered it through a piece of a very thick linen-cloth doubled; by which means it was rendered free of any crusts, and equally pellucid with fountain-water. With this lime-water I filled the above *vessel*, and found its weight to be 25356 grains*.

(c) MONDAY, at seven in the evening, I poured upon a pound of calcined oyster-shells, fresh from the fire, ten times their weight of water; next morning at ten I decanted off the clear lime-water; and having filtered it, as above, filled the *vessel* with it; it weighed 25397 grains.

(d) TUESDAY at mid-day, I poured seven pounds of the single lime-water, (c), upon one pound of calcined oyster-shells, newly taken from the fire, stirring them well for some time after; at three quarters past six in the evening, I decanted off, and filtered as above, the clear lime-water; and having filled the *vessel* with it, found its weight to be 25457 grains.

HENCE it appears, that 100 cubical inches of the lime-water (b) exceeds, in weight, that quantity of fountain-water by 36 grains: (c) exceeds it by 77 grains, and (d) by 137 grains.

THE specific gravity therefore of the weak lime-water (b) is to that of fountain-water nearly as 704 to 703. The specific gravity of the single lime-water (c) is to that of common water nearly as 329 to 328; and the double lime-water (d) is in specific gravity to water nearly as 186 to 185.

IT is observable that the specific gravities of the single and double lime-waters (c) and (d), are considerably less than the specific gravities of the single and double lime-waters (a), and A. and B. mentioned

may have arisen from our having put a few more drops of water into the vessel than Mr Gray did. But although, in weighing fluids with this vessel, one might err six times more than this, yet it would not affect the point we have in view, which is not to determine with the greatest accuracy the different specific gravities of different lime-waters, but *only* to shew that they are *different*.

* The oyster-shells made use of in this, and the following experiments, were got from among the rubbish on the south side of the castle of Edinburgh, and were quite free of any sea-salt.

tioned *p.* 357. of my Essay on the virtues of lime-water, &c. But, if it be considered, that, in making the latter, a much less proportion of water was added to the quick-lime than in making the former, it will appear that this difference of their specific gravities does not infer any thing against the accuracy of the hydrostatic balance; but clearly shews, that the strength of lime-water varies according to the quantity of water poured on the quick-lime.

It may be worth while to observe, that the specific gravities of the lime-waters (*b*), (*c*), and (*d*), did not differ more than their tastes. The first was weakest and least disagreeable; the second was stronger; and the third still stronger, and somewhat pungent. Further, while the double lime-water (*d*) gave, in a few minutes, a copper-colour to silver, the weak lime-water (*b*) produced no sensible change upon it.

II. HAVING, formerly, found that lime-water and claret wine, mixed together, in a certain proportion, acquired a colour like that of gun-powder*: I thought, that, by mixing claret with different lime-waters, one might judge whether they were all equally strong of the lime or not. The result of the experiments was, that one tea-spoonful of claret required four tea-spoonfuls of the lime-water (*b*); two and about one third of (*c*); and one and a half of (*d*), to give it the full gun-powder colour. These experiments, though not so accurate as those made with the balance, yet clearly demonstrate a remarkable difference of strength betwixt the above lime-waters.

III. TWENTY grains of salt of tartar, being mixed with eight ounces and two drams of the weak lime-water (*b*), after it had stood five days on the lime, the mixture became immediately white and turbid, and soon precipitated a white powder; which, being separated from the water, by filtration, and dried, weighed $2\frac{2}{3}$ gr.

THE same quantity of salt of tartar, mixed with eight ounces and

* Essay on the virtues of lime-water, *p.* 368.

and two drams of double lime-water, that had stood eight days on the lime, became considerably thicker and whiter than the former; and afforded rather more than seven grains of white powder.

THE same quantity of salt of tartar being mixed with eight ounces and two drams of the double lime-water (*d*), which had stood 24 hours on the lime, gave eight grains of a white powder.

IT was observable, that these three lime-waters retained the taste of the lime, after being mixed with the salt of tartar, and this equally after precipitation as before it.

SINCE the earthy powder precipitated by these different lime-waters, proceeds *wholly*, or *almost* wholly, from the waters, and not from the fixed alkaline * salt; these experiments shew, beyond doubt, that double lime-water may contain thrice as much lime as lime-water made by pouring on quick-lime 512 times its weight of water.

IV. 1. MONDAY 24th December, at eight in the evening, I poured upon a dram of fresh calcined oyster-shells, reduced to a powder, 520 drams of boiling water.

2. AT the same time, I poured upon a pound of the same calcined shells, eight pounds of boiling water.

3. TUESDAY, at eleven before noon, I poured fifty ounces of the lime-water, N° 2. on nine ounces of fresh calcined oyster-shells; and, at eight in the evening, I filtered, through brown paper, these three waters, and put sixteen ounces of each of them into a basin by itself; and, having placed the basins in a closet, where they might be pretty free from dust, I let them stand nineteen days. After this, I filtered the several waters thro' brown paper, and having collected the earthy crusts, and dried them well, I found, that N° 1. afforded very near four grains, N° 2. near 12 grains, and N° 3. rather more than 13 grains.

ALTHO' these three lime-waters had, at the time they were filtered,

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* What proves this is, that the calcarious matter, precipitated by mixing salt of tartar with lime-water, is greater or less, in proportion to the strength and quantity of the lime-water; but not in proportion to the quantity of the salt. Thus 12 grains of salt of tartar, mixed with four ounces of strong lime water, yielded as much of this matter as the same quantity of this lime-water, mixed with 18 grains of the salt.

trated, quite lost their taste; yet, observing that N° 2. and 3. became turbid when mixed with salt of tartar, I added eight grains of this salt to twelve ounces of these two waters; and the white powder which was precipitated, when dried, weighed just one grain and a half.

HAVING filtered the lime-waters of N° 2. and 3. into the same bottle, before I suspected that any thing of the lime remained in them, it became impossible to know, which of them afforded most of the calcarious powder precipitated by the alkaline salt, or whether it did not proceed wholly from N° 3.; in which case sixteen ounces of it must have contained 17 gr. of the earthy part of the lime, and N° 2. only 12 gr.

SINCE N° 2. and 3. were not quite free of the lime, altho' they had stood exposed to the open air 19 days, and had lost above $\frac{1}{4}$ by evaporation; it follows, that the surest way of knowing the quantity of calcarious earth contained in lime-water, is to evaporate it, as Dr Langrish did*: and if it be objected to this, that all water affords some earth when evaporated, the quantity of this may be determined by experiment: tho' in many waters, it may well be neglected, on account of its smallness.

IT has been argued, that quick-lime must, after many repeated affusions of water, yield as strong lime-water as at first; because, as long as there remains any virtue in the lime, the water will extract it, and continue to do so, till it has taken up as much of the lime as it can bear. But to this we cannot agree: for, tho' there is undoubtedly a certain degree of strength which lime-water can never exceed; yet, in order to communicate to water this degree of strength, flaked lime may not only be insufficient, but repeated additions of quick-lime may be necessary; unless perhaps a very small proportion of water is poured upon it. Quick-lime, fresh from the fire, yields its virtues more easily, than when weakened by long exposition to the air, or by many affusions of water: the water must extract the virtues of the latter, while the former, by a sort of explosive force of its own, quickly impregnates the water. Nor

* Physical Experiments on brutes, p. 11.

is it to be wondered at, that quick-lime, fresh from the fire, should, at first, impregnate water more strongly with its virtues than it does afterwards. This is as easily conceived, as that boiling water should extract more of the virtues of tea or coffee than cold water. The only difference is, that the *menstruum* in the latter case acts more powerfully, while in the former the substance to be extracted affords its finer parts more readily, and in greater abundance.

UPON comparing the experiments N^o I. with those of N^o III. and IV. it appears, that the difference between the specific gravities of different lime-waters and common water, is much more than the weight of the calcarious matter contained in these lime-waters: there must, therefore, be something else besides this earthy matter which quick-lime communicates to water, by which its weight is increased*. Perhaps quick-lime may also, in some other way unknown to us, alter the specific gravity of water. But whatever may be in this, it is evidently unreasonable to deny, that lime-water is as much specifically heavier than common water, as the hydrostatic balance or other accurate experiments shew; because we cannot account for this excess of gravity from any thing we know of the contents of lime-water. This is no less unphilosophical, than if one was to doubt of universal gravity, because philosophers have hitherto attempted, in vain, to account for it. If we mistake not the matter much, the contrary has always been the opinion of mankind, *viz.* that every well attested fact is to be believed, altho' we are ignorant of its cause, or cannot shew the particular way in which it happens.

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ENOUGH,

* As lime-water, after its earthy part has been precipitated by an alkaline salt, continues to taste strongly of the lime, it follows, that, besides this earth, it contains some more active and subtile part, to which its taste and virtues are chiefly owing: for we know that the calcarious matter of lime-water is perfectly insipid and void of any other virtue than what all absorbent earths possess. This active and more subtile part of lime-water seems to be separated from its earth by the alkaline salt, which strongly attracts and embraces it. And hence lime-water mixed with salt of tartar does not lose its taste of the lime, by being exposed to the open air. Does not a solution of a fixed alkaline salt in water, poured on quick-lime, separate this subtile active matter of quick-lime from its earthy part, by strongly attracting it? And do not soap-leys consist of water and fixed alkaline salt united with this active part of quick-lime, without any, or almost any, of its earthy part?

ENOUGH, it may, perhaps, be thought more than enough, has been said, to shew, that the strength of lime-water is very different, according to the different quantities of water poured on quick-lime. However, I must be allowed to say, that this point, which has been disputed by my good friend, is of that consequence as to deserve to be fully cleared up; since, to such as drink lime-water, with a view to the cure of the stone, it is of no small importance to know, how it may be prepared so as to have the surest and speediest effects. And as lime-water, injected into the bladder, will undoubtedly dissolve a stone lodged there; it is evident, that, after the bladder has been accustomed to the weaker lime-waters, or to those even softened with a little sweet milk, the dissolution of the stone may be much hastened, by injecting such as are more strongly impregnated with the virtues of the lime.

WITH regard to the lithontriptic powers of oyster-shell and stone-lime water, I shall only say, that, as in a variety of experiments made during the course of ten years, I had always observed the superior efficacy of the oyster-lime water, I thought it to no purpose to make a new trial: any one who doubts on which side the truth is, may easily satisfy himself. But, in making the experiment, the *calculi* should either be immersed in a large quantity of lime-water, or else it should be renewed upon them every three or four days.

POSTSCRIPT TO THE WALPOLE CASE

A LETTER to the Rev. THOMAS BIRCH, D. D.
Secret. R. S. from JOHN PRINGLE, M. D. F. R. S. in-
closing two papers communicated to him by ROBERT
WHYTT, M. D. F. R. S.

S I R *,

Pallmal-Court, St James's, Dec. 10. 1757.

ABOUT three weeks ago I put into your hands an extract of a letter I had then received from Dr Whytt, containing a postscript to his observations on Lord Walpole's case, and slightly mentioning some doubts he had then about the justness of Dr Springfield's experiments with lime-water, from some trials he himself had made, upon reading that gentleman's curious treatise on the extraordinary lithontriptic quality of the waters at Carlsbad in Bohemia. Within these few days, Dr Whytt having favoured me with a full account of those experiments, I have herewith sent you his paper; in order, if you please, to lay it before the society; which the author desires may be done, in case these observations should be judged useful.

THE other paper inclosed was sent me by the same hand, to be likewise presented to the society, as a well-attested instance of the electrical power in the cure of a palsy. To the other testimonies I have subjoined what Dr Whytt says in his letter to me, by way of strengthening the evidence. I shall only add, that since Mr Brydone, the author of this account, has omitted telling how long the patient has continued in perfect health since the operation, it appears she must have been well for some months before the date of his paper; because, before the end of last summer, Dr Whytt transmitted the same case to me, which I then returned, in order to have it drawn up in a fuller manner, and with other vouchers besides the gentleman who performed the cure. The Doctor has been so good as to comply with my request, having procured a more ample account.

* First published in the Phil. Transf. and read December 15. 1757.

478 POSTSCRIPT TO L. WALPOLE'S CASE.

count of the circumstances from Mr Brydone, and the attestation of two ministers, besides that of the patient herself *. My difficulties being thus removed, I believe I may now with freedom offer this very curious case to the attention of the society. I am,

S I R,

Your most obedient humble servant,

JOHN PRINGLE.

POSTSCRIPT to Dr WHYTT'S observations on Lord WALPOLE'S CASE *.

“ I Do not know, if it be worth while to observe, that lately, in
 “ making some experiments with different *calculi*, there was one
 “ almost as white as chalk, but of a less hard substance than the others;
 “ and which was not in the least degree dissolved or softened by be-
 “ ing infused 20 days in oyster-shell lime-water, but yielded some-
 “ what to a solution of Spanish soap in common water.

“ FROM this experiment one may conclude, that it is better to pre-
 “ scribe both soap and lime-water for the stone, than any one of them
 “ alone;

* After this paper was read at the society, Dr Pringle having acquainted Dr Whytt, that Mr Patrick Brydone had omitted, in his account, the name of the parish where the woman lived, the time when she was cured, and also that he had not fully dated his paper; Dr Whytt some time after wrote to Dr Pringle, that having desired Mr Brydone to furnish him with these particulars, he had received for answer, “ That the woman, on whom the cure
 “ was performed, had lived all her life in the parish of Coldinghame, and for the last twelve
 “ years in that town: That her father had died of the palsy seven years ago, after having
 “ been subject to that distemper for several years: That the cure was performed in his father's
 “ house at Coldinghame, on the 4th, 5th, 6th, and 11th Days of April 1757, a circum-
 “ stance he had noted down: That as to the date of his paper, presented to the Royal socie-
 “ ty, he only recollects it was written some day in the beginning of November last: but as
 “ the woman still continued well, he hoped the precise day of the month was no material
 “ omission.” This letter to Dr Whytt is dated, Coldinghame, January 9. 1757.

* Read December 8. 1757.

“ alone; and that, if one of these remedies has failed of giving relief,
 “ the other ought to be tried: for as the above white *calculus*, which
 “ yielded a little to the solution of soap, resisted lime-water; so
 “ there may perhaps be others that are readily dissolved by lime-
 “ water, but little affected by soap.

“ Dr Springsfeld’s experiments with lime-water are somehow not
 “ just; for in several *calculi* I have found the dissolving power of
 “ oyster-shell lime-water above eight times greater than he makes
 “ it.”

SOME OBSERVATIONS on the Lithontriptic Virtue of the
 Carlsbad waters, lime-water, and soap: In a letter to Dr JOHN
 PRINGLE, F. R. S. from Dr ROBERT WHYTT, F. R. S. and Pro-
 fessor of medicine in the university of Edinburgh.

S I R *,

FROM the experiments related in Dr Springsfeld’s *Commentatio
 de prerogativa thermarum Carolinarum, &c.* which you were so
 good as to send me some time ago, it appears, that these waters are
 not only possessed of a very extraordinary power of dissolving the
 stone, but that in this respect they greatly exceed lime-water.

(A) THUS, Dr Springsfeld having infused, for 14 days, in a heat
 of 96 degrees of Fahrenheit’s scale, three pieces of the same *calculus*,
 each weighing 30 grains, in eggshell-lime-water, the Carlsbad wa-
 ter, and in the urine of one who daily drank this last water, re-
 newing these several menstrua every day, he found, on the 15th
 day, that the *calculus* in the lime-water had lost 1 grain, the *calculus*
 in the Carlsbad water 6 grains, and that in urine 5 grains.

(B) AGAIN, having divided another *calculus* into four parts,
 each of which was reduced to 80 grains, he put the first in oi-
 ster-shell lime-water, the second in Carlsbad water, and the third in
 the urine of a person who drank this water. After 20 days, during
 which time the menstrua were renewed every day, and kept in

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* Read, Dec. 15. 1757.

a heat of 96 degrees, the dried *calculi* had lost of their weight as follows: the first 3 grains, the second 18 grains, and the third 14 grains.

ALTHOUGH I make no doubt that Dr Springsfeld, who appears to be a man of candour, as well as learning, has faithfully related the event of the experiments which he made; yet either the lime-water he used must have been very weak, or some other mistake must have happened in his experiments: for in all the numerous trials I made, about 15 years ago, of lime-water as a solvent for the stone, I always found its dissolving power much greater than it appears in Dr Springsfeld's experiments. And as in these trials different urinary stones were used, it can scarcely be imagined, that it was owing to the peculiar hardness of Dr Springsfeld's *calculi*, that the lime-water made so little impression on them. However, to be still further satisfied of this matter, I made the following experiments.

1. I put a piece of a very hard *calculus*, which I shall call *x*, weighing 80 grains, in oyster-shell lime-water, renewing the lime-water every day, and keeping it in a heat between 90 and 106 degrees of Fahrenheit's scale. After 20 days, I took out the *calculus*; and having set it by for some days, till it was become quite dry, I brushed away all the rotten part of it, which was reduced to a kind of chalky powder, and found that the undissolved part of it weighed 57 grains.

2. AT the same time a piece of another *calculus*, *z*, weighing 15 grains, was, after a like infusion of 20 days in oyster-shell lime-water, reduced to 10 grains.

3. I put a piece of *z*, weighing 14 grains, in a solution of half an ounce of the internal part of Spanish soap in nine ounces of water, and every third day renewed the solution, which was kept in a heat of about 60 degrees. After 14 days, I found the undissolved part not to exceed 11 grains.

4. A piece of a white chalky *calculus*, *y*, weighing 30 grains, had
near

near 4 grains of its substance dissolved, by being 14 days infused as above in a solution of soap.

FROM N^o. 1. above, compared with Dr Springsfeld's exper. (B), it appears, that the dissolving power of oyster-shell lime-water is to that of the Carlsbad water as 23 to 18, supposing the *calculi* used in these experiments to have been equally easy to dissolve.

N^o 3. compared with Dr Springsfeld's exper. (A), shews, that the dissolving power of a solution of the inner part of Spanish soap in a heat of 60 degrees, is to that of the Carlsbad water, in a heat of 96 degrees, as 15 to 14.

FROM N^o 4. compared with (A), the dissolving power of soap is to that of the Carlsbad water only as 4 to 6; but it is probable, that had the solution of soap been kept in a heat of 96 degrees, its dissolving power would, even in this experiment, have nearly equalled that of the Carlsbad water. It may, perhaps, be worth while to observe, that a piece of the white chalky *calculus* of N^o 4. was not in the smallest degree dissolved by lying in lime-water 20 days.

5. IN exper. 19. of my Essay on the virtue of lime-water, a piece of *calculus*, *b*, weighing 31 grains, lost 7 grains by being infused 36 hours, in a heat of above 100 degrees, in very strong oyster-shell lime-water. And in the same water, of a moderate strength, another piece of *b* lost, in the same time, 5 grains.

IN this last experiment, the lithontriptic virtue of lime-water appears to be stronger than in N^o 1. and 2. above; and greatly exceeds that of the Carlsbad water in Dr Springsfeld's exper. (A) and (B.)

BUT altho', from what has been said, it appears not only that lime-water, but also a solution of soap, dissolves the stone in close vessels as fast, nay faster than the *thermæ Carolinæ*; yet these last waters, when the *calculi* were so placed in open vessels, that the water from the fountain might constantly flow along them, effected a much quicker dissolution than lime-water or even soap-ley, or

indeed any known *menstruum*, except, perhaps, strong spirit of nitre: for, in the first experiment made by Dr Springsfeld, a *calculus* of two ounces and a half was, in this manner, quite dissolved in six days. From this experiment, compared with that of Dr Springsfeld mentioned above (B), it will be found, upon calculation, that the dissolving power of the Carlsbad water, when it is allowed to flow constantly from the fountain along the stone, is nearly 39 times greater than when it is only poured fresh on the *calculus* once a-day. What may have been the reason of this surprising difference of the lithontriptic power of the Carlsbad water in these different circumstances, I will not pretend to say. I think it can scarcely be accounted for from the gentle motion of the water along the surface of the *calculus*. Was it then owing to some very volatile active part, which the water quickly loses, after being taken from the fountain?

BUT how great soever the dissolving power of the Carlsbad waters may be, when they issue from the bowels of the earth; yet that they do not communicate a much greater dissolving power to the urine than lime-water, will appear from comparing the two following experiments.

IN Dr Springsfeld's exper. (A) above, the urine of a person who drank the Carlsbad waters, reduced, in 14 days, a piece of *calculus*, weighing 30 grains, to 25 grains. And in an experiment made by Dr Newcome, now Lord Bishop of Llandaffe, who drank four English pints of oyster-shell lime-water daily, his Lordship's urine reduced, in four months, a piece of *calculus*, weighing 31 grains, to three small bits, weighing in all six grains *. Whence it follows, that the dissolving power of his Lordship's urine must have been to the dissolving power of the urine of the person who drank the Carlsbad waters nearly as 35 to 65. But if we consider, that the *calculus* infused in the urine of the person who drank the Carlsbad waters was kept always in a heat of 96 degrees, while in Dr Newcome's experiment, which was made during part of the autumn and winter, no artificial

* Essay on lime water, p. 455.

artificial heat was used, it will appear probable, that the dissolving power of his Lordship's urine was little inferior to that of the person who drank the Carlsbad waters; for lime-water, in a heat of 96 degrees, dissolves the *calculus* at least twice as fast as in the common heat of the air in winter. Further, if it be attended to, that the quantity of Carlsbad waters drank every day before dinner is from six to eight lib. while his Lordship only drank four lib. of lime-water in 24 hours, it will follow, that whatever the different dissolving powers of the lime-water and Carlsbad waters may be out of the body, yet the former seems, in proportion to the quantity drank, to communicate at least an equal dissolving power to the urine.

BUT without presuming to decide certainly, as to the comparative virtue of the Carlsbad waters and lime-water, I shall conclude with observing, that tho' the Carlsbad waters are less disagreeable to the taste, and may be drank in larger quantity than lime-water, yet this last may be drank equally good in all places, and at all seasons of the year; which is not the case with the Carlsbad waters.

November 30. 1757.

An instance of the ELECTRICAL VIRTUE in the cure of a PALSY.

By Mr PATRICK BRYDONE *.

ELIZABETH FOSTER, aged 33, in poor circumstances, unmarried, about 15 years ago was seized with a violent nervous fever, accompanied with an asthma, and was so ill, that her life was despaired of. She recovered, however, from the violence of her distemper; but the sad effects of it remained. For, from this time, she continued in a weakly uncertain state of health, till the month of July 1755, when she was again taken ill of the same kind of fever; and after it went off, she was troubled with worse nervous symp-

P p p 2. toms.

* Read December 15. 1757.

toms than ever, ending at last in a paralytic disorder, which sometimes affected the arm, sometimes the leg, of the left side, in such a manner as that these parts, though deprived of all motion for the time, yet still retained their sensibility. In this condition she remained till the spring 1756, when unexpectedly she grew much better; but not so far as to get quite rid of her paralytic complaints, which, in cold weather, seldom failed to manifest themselves by a numbness, trembling, sensation of cold, and a loss of motion in the left side.

THIS paralytic tendency made her apprehensive of a more violent attack; which accordingly soon happened: for, about the end of August, in the same year, her symptoms gradually increased, and, in a very short time, she lost all motion and sensation in her left side. In this state she continued throughout last winter, with the addition of some new complaints; for now her head shook constantly; her tongue faltered so much, when she attempted to speak, that she could not articulate a word; her left eye grew so dim that she could not distinguish colours with it; and she was often seized with such an universal coldness and insensibility, that those who saw her at such times scarce knew whether she was dead or alive.

WHILST the woman was in this miserable condition, observing that she had some intermissions, during which she could converse and use her right leg and arm, in one of those intervals I proposed trying to relieve her by the power of electricity. With this view, I got her supported in such a manner as to receive the shocks standing, holding the phial in her right hand, whilst the left was made to touch the gun-barrel. After receiving several very severe shocks, she found herself in better spirits than usual; said she felt a heat, and a prickling pain in her left thigh and leg, which gradually spread over all that side; and after undergoing the operation for a few minutes longer, she cried out, with great joy, that she felt her foot on the ground.

THE electrical machine producing such extraordinary effects, the action was continued; and that day the woman patiently submitted to receive above 200 shocks from it. The consequence was, that the shaking

shaking of her head gradually decreased, till it intirely ceased; that she was able at last to stand without any support; and on leaving the room, quite forgot one of her crutches, and walked to the kitchen with very little assistance from the other. That night she continued to be well, and slept better than she had done for several months before, only about midnight she was seized with a faintishness, and took notice of a strong sulphureous taste in her mouth; but both faintness and that taste went off, upon drinking a little water. Next day, being electrified as before, her strength sensibly increased during the operation, and when that was over, she walked easily with a stick, and could lift several pounds weight with her left hand, which had been so long paralytic before. The experiment was repeated on the third day; by which time she had received in all upwards of 600 severe shocks. She then telling us that she had as much power in the side that had been affected as in the other, we believed it unnecessary to proceed farther, as the electricity had already, to all appearance, produced a compleat cure. And indeed the patient continued to be well till the Sunday following, *viz* about three days after the last operation; but upon going that day to church, she probably caught cold; for upon Monday she complained of a numbness in her left hand and foot; but, upon being again electrified, every symptom vanished, and she has been perfectly well ever since.

Coldingham, Nov. 1757.

PATRICK BRYDONE.

THAT the above is a true and exact account of my case, and of the late wonderful cure wrought on me, is attested by

ELIZABETH FOSTER.

I was eye-witness to the electrical experiments made by my son on Elizabeth Foster, and saw with pleasure their happy effects. By the blessing of God accompanying them, from a weak, miserable,
and

and at sometimes almost an insensible state, she was, in a very short time, restored to health and strength; of which the above is, in every respect, a true account.

ROBERT BRYDONE,
Minister of Coldingham.

Extract of a letter from Dr WHYTT to Dr PRINGLE, relating to this account: Dated Edinburgh, 1st Dec. 1757.

SOME days ago, I had transmitted to me Mr Brydone's account (inclosed) of the success of the electrical shocks in a paralytic patient, attested by the patient herself, and by Mr Brydone's father, who is minister at Coldingham, in the shire of Berwick. At the same time I had a letter from the Reverend Mr Allan, minister of Eyemouth, (in the neighbourhood), informing me, that he had examined the patient particularly, and found Mr Brydone's account to be perfectly true. He further informs me, that he never observed the electrical shock so strong from any machine as from Mr Brydone's. It seems, that gentleman has not only applied himself to the study of natural philosophy, but also of medicine.

ROBERT WHYTT.

OBSER-

OBSERVATIONS

ON THE

NATURE, CAUSES, and CURE

Of those DISORDERS which are COMMONLY called

NERVOUS, HYPOCHONDRIAC,
or HYSTERIC;

Συμπαθία πάντα. Hippocrat. de aliment. § 4.

To which are prefixed some Remarks on the SYMPATHY of the NERVES.

The first Edition published in the 1764, the Second in the 1765, and the Third in the 1767.

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P R E F A C E.

THE disorders which are the subject of the following Observations have been treated of by authors, under the names of Flatulent, Spasmodic, Hypochondriac, or Hysterical. Of late, they have also got the name of NERVOUS; which appellation having been commonly given to many symptoms seemingly different, and very obscure in their nature, has often made it to be said, that physicians have bestowed the character of *nervous*, on all those disorders whose nature and causes they were ignorant of. To wipe off this reproach, and, at the same time, to throw some light on nervous, hypochondriac, and hysterical complaints, is the design of the following observations; which are also intended to shew, how far the principles laid down in my Essay on the vital and other involuntary Motions of Animals, may be of use in explaining the nature of several diseases, and consequently, in leading to the most proper method of cure*.

SINCE, in almost every disease, the nerves suffer more or less, and there are very few disorders which may not, in a large sense, be called *nervous*, it might be thought that a treatise on nervous diseases should comprehend almost all the complaints to which the human body is liable. The design, however, of the following Observations is far different. In them, it is only proposed to treat of those disorders which in a *peculiar* sense deserve the name of *nervous*, in so far as they are, in a great measure, owing to an uncommon delicacy or unnatural sensibility of the nerves, and are therefore observed chiefly to affect persons of such a constitution.

As many of these complaints depend upon that sympathy which obtains between the various parts of the body, it seemed necessary to begin with some observations on the *sympathy* of the *nerves*; a subject of the greatest importance in pathology!

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IN

* See above, p. 207. 208.

P R E F A C E.

IN reasoning on the nature and causes of nervous disorders, I have endeavoured to avoid uncertain *hypotheses*; and therefore have had no recourse to any imaginary *flight, repercussion, dispersion, confusion*, or *jarring contest* of the animal spirits; for whose existence we have only probability, and of whose peculiar nature and properties we are altogether ignorant. But altho' the minute structure of the nerves, the nature of their fluid, and those conditions on which depend their powers of feeling, and communicating motion to the body, lie much beyond our reach; yet we know certainly, that the nerves are endued with feeling, and that as there is a general sympathy which prevails through the whole system; so there is a particular and very remarkable *consent* between various parts of the body. From this sentient and sympathetic power of the nerves, I have endeavoured to deduce the various symptoms of the nervous kind; and have thought it better to stop short here, than to amuse myself or others with subtile speculations concerning matters that are involved in the greatest obscurity.

IF it should be said, that to account for diseases from the sensibility or sympathy of the nerves, while we know not wherein these powers consist, is no better than referring them to a *facultas incognita*, or to the hypothetical *motions* and *countermotions* of the animal spirits; I shall only answer, that altho' we cannot explain why grief or joy should, by means of the nerves, excite a greater motion than usual in the vessels of the lachrymal glands; yet it is leading us to the truth, and advancing one step farther in our knowledge, to shew that the increased secretion of tears, occasioned by those passions of the mind, proceeds from this cause, and not from any compression of the lachrymal glands or their ducts, by the neighbouring muscles, as has been commonly imagined: And, to ascend from small things to great, altho' Sir Isaac Newton did not pretend to explain the cause of gravity, yet he made no small improvement in physical astronomy, when, from this principle alone, he accounted for the various motions

P R E F A C E.

motions of the planets, and banished the imaginary *vortices* of Des Cartes, which had been contrived, but unsuccessfully, to explain the *phænomena* of the solar system.

IN the practical part, I have confined myself chiefly to what experience had suggested ; and have only advised such remedies as I have used with success myself, or had recommended to me by those whom I could trust.

As many of the subjects I have treated of are not a little obscure, it cannot be expected that they should admit of as clear an explanation as matters that are less intricate ; and this, it is hoped, will make the learned in the profession over-look with candor the defects which may be found in the following observations. However, if with all their imperfections, they shall be of any use in conveying, to the younger and less experienced physicians, a clearer notion of the nature of those disorders which have been *commonly* called nervous, hypochondriac, or hysterical, or in any case direct them more happily in the cure, I shall have my aim, and be sufficiently rewarded in the satisfaction of thinking that my labour has not been altogether useless to the public, the good of which ought to be the principal view of every writer.

Hoc opus, hoc studium, parvi properemus et ampli,
Si patriæ volumus, si nobis vivere chari.

Edinburgh, Nov. 15.

1764.

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T H E

REIGN OF KING CHARLES THE FIRST

IN WHICH ARE CONTAINED THE MOST IMPORTANT AND INTERESTING PARTS OF HIS REIGN, FROM HIS MARRIAGE TO HIS DEATH.

By JOHN BURNET, D.D. Bishop of Salisbury.

LONDON, Printed by J. Streater, at the Sign of the Gun, in St. Dunstons Church-yard, 1682.

THE HISTORY OF THE REIGN OF KING CHARLES THE FIRST, FROM HIS MARRIAGE TO HIS DEATH.

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C H A P. I.

Of the STRUCTURE, USE, and SYMPATHY of
the NERVES.

BEFORE we enter upon the subject of the following observations, it may be proper to make a few remarks concerning the structure, use, and sympathy of the nerves.

1. THE nerves are those small cords, which rising from the brain and spinal marrow, are distributed to every part of the body. They appear to be no more than continuations of the medullary substance of the parts from whence they proceed, and owe their strength and firmness to the membranes and cellular texture which surround them.

2. THE larger nerves (1.) are evidently composed of many smaller ones, which run parallel to each other, and seem to be quite distinct from their origin to their termination, without any such communications between their branches as are observed every where in the system of arteries and veins.

3. THE smallest nervous filaments that can be traced by dissection are still composed of lesser threads; so that we can have no idea of the exility of a single nervous fibril.

4. ALTHO' it seems probable that the nerves (3.), which are continuations of the medullary substance of the brain and spinal marrow, derive from thence a fluid; yet the extreme smallness of the nervous tubes, and the subtility of that fluid which they contain, make us altogether ignorant of its peculiar nature and properties. Nor do we know, certainly, whether this fluid serves only for the nourishment and support of the nerves, or whether it be not the *medium* by which all their actions are performed.

5. THE nerves communicate sense and a power of motion to the body.

SINCE *opium*, without entering the blood, or being carried to the several parts of the body, lessens or destroys their powers of feeling and motion, merely by acting on the extremities of the nerves to which it is applied *, it follows, that the nerves must be the instruments of sensation, and necessary for performing motion. Many other experiments and arguments might be mentioned to the same purpose; but as this agency of the nerves is a point generally acknowledged, it would be unnecessary to enter here into a more particular proof of it.

6. ALTHO' every part of the body furnished with nerves, has either more or less of feeling †; yet there are only some of those parts whose structure renders them capable of motion, *viz.* the muscles, and such organs as are in part muscular; and the blood-vessels, which, from the effects of blisters and other *stimuli*, appear evidently to be possessed of irritability, or a power of alternate contraction.

7. THERE are only two kinds of motion observed in the bodies of living animals, *viz.* voluntary, and involuntary from *stimuli*. In order to the performance of the first, the nervous power is not only necessary, but also a free communication, by means of the nerves, between the brain and the parts to be moved. The second continues for some time, though in a much weaker degree, even in those muscles whose connection with the brain is wholly cut off; whence it has been concluded, that this kind of motion is independent of the nerves, and owing to some power or property in the muscular fibres themselves, or in the glutinous matter connecting the elements of which they are composed ‡. That this conclusion is not, however, well founded, I have formerly shewn by several arguments ||; and shall, therefore, at present only observe, that, since a solution of *opium*, by affecting the extremities of the nerves to which it is applied, and without being received into the blood, or carried to

* See above, p. 321. 324. &c.

† There are some exceptions to this, such as bones and cartilages, which, tho' not destitute of nerves, are yet, in a natural state, insensible.

‡ Acta Gotting. vol. 2. p. 152. &c.

|| See above, p. 324.

to the brain or muscles, destroys not only the power of voluntary motion in animals, but also renders their muscles incapable of being excited into contraction by the strongest *stimuli* * ; it evidently follows, that involuntary, as well as voluntary motion depends upon some power or influence of the nerves †.

BUT be this as it will ; from the continuance of the motion of the heart, and other muscles, after they are separated from the body, one may safely conclude, that the contraction of irritated muscles is owing to the distension of their hollow fibres, by a more copious influx of the nervous fluid at that time. Does this fluid act in some other way than by distending the muscular fibres ? or is it only necessary to keep them in a proper state for being acted upon by that living principle from which all their motions are to be derived ‡ ?

Q q q 2

8. As

* See above, p. 199.

† See above, p. 321. 324.

‡ Since it has been the prevailing opinion among physiologists, that the contraction of the muscles is owing to the dilatation of their hollow fibres, by a greater influx of the nervous fluid into them, it may not be amiss, briefly, to mention the principal arguments which render this opinion, at least, very improbable.

1. As far as we can judge from experiments, the muscles become less bulky in a state of contraction than they were before. The hearts of frogs, when taken out of the body, become really less every time they contract, and their contractions seem not to be owing to an inflation of their fibres or vessels, but to the particles of which they are composed approaching nearer to one another, and running into closer contact.

2. The extraordinary smallness of the nerves, and the very slow secretion and motion of their fluid, makes it improbable that muscular motion is owing to the distension of the fibres of the muscles by a sudden influx of that fluid. Nor have we any reason to think, that the nervous juice dilates the muscular fibres by means of any rarefaction or effervescence.

3. The muscles grow more tendinous by age, *i. e.* their fibres towards their extremities degenerate into solid threads ; and this happens soonest when the muscles are much used : but if the contraction of a muscle were owing to the inflation of its fibres by any fluid, the more frequently it was moved, it would be the less apt to become tendinous.

4. If the muscular fibres are hollow, and of a cylindrical form, or made up of vesicles, it may be demonstrated, that they cannot be rendered, in the first case, above $\frac{1}{7}$, and in the second above $\frac{1}{2}$ part shorter, by the influx of a fluid into their cavities ; but we know that the difference between the *sphincter pupillæ* and *ani*, and the stomach and bladder, in their greatest state of contraction and distension, is much more than any of these proportions.

5. The regular alternate contraction of the hearts of frogs, for five or six hours after decollation and the destruction of their spinal marrow, and for half an hour or more after they are

8. As the nerves are continuations of the medullary substance of the brain and spinal marrow, it is probable that they are partly nourished by those vessels, which are spread on that production of the *pia mater* which surrounds them, in like manner as the brain derives its nourishment from the arteries of the *pia mater*. If this be true, we may readily see why the nerves lose their powers, when they are wholly deprived of the arterial blood; and, on the other hand, retain them, in some measure, after the brain is ossified or petrified.

It has been thought by some, that nutrition is chiefly performed by means of the nervous fluid, because those parts whose nerves are destroyed, or wholly deprived of their usual power, are observed to become smaller.

BUT whoever considers the inconceivable exility of the nerves, and how slowly any fluid must be derived by their means from the brain *, will scarcely think that the nutrition and growth of the body can be performed in this way. Nor does the withering of such muscles as are totally palsied prove, that nutrition is owing to the nervous fluid; for we know, from certain experiments and observations, that the motion of the fluids in the very small vessels depends, in a great measure, upon the influence of their nerves; and that, when this is wanting, the fluids either do not circulate at all thro' those vessels, or, at least, in a very languid manner: whence the parts, to which they belong, collapse, and are not properly nourished †.

9. OUR bodies are, by means of the nerves, not only endowed with feeling, and a power of motion, but with a remarkable sympathy,

are separated from their bodies, clearly proves, that an influx of a fluid from the nerves into the muscles is not necessary for their contraction: for although the spirits remaining in the nerves of the heart should be supposed to occasion a few contractions of this muscle, yet they cannot be sufficient for producing several thousand distensions of its hollow fibres.

The above arguments, if they do not entirely overthrow the common opinion, will, at least, justify me in not having recourse, in the following treatise, to the irregular motion, increased derivation, repercussion, confusion, or hurry of the animal spirits, in accounting for the symptoms of nervous, hypochondriac, and hysterical disorders.

* See above, p. 221, &c.

† Ibid. p. 234. and 235.

pathy, which is either general, and extended through the whole system, or confined, in a great measure, to certain parts.

10. THAT every sensible part of the body has a sympathy with the whole, will sufficiently appear from the following facts.

COLD water thrown on any part of the body that is warm produces a sudden contraction of the whole vessels and pores of the skin, and by that means frequently puts a stop to small hæmorrhages. The *effluvia* of certain substances when smelled to, instantly communicate new life and vigour to the whole body, while others affect some delicate women with fainting and convulsions.—By means of different musical sounds, various passions may be excited or calmed, and diseases are said to have been sometimes cured *. By doleful stories, or shocking sights, delicate people have been often affected with fainting and general convulsions †.

WHEN the brain is wounded, inflamed, suppurated, or otherwise hurt, almost every part of the body is liable to suffer, and vomitings, tremors, convulsions, palsies, &c. often ensue. In animals newly dead, the whole muscles of the trunk and extremities are strongly convulsed, when a probe is pushed down through the spinal marrow.

WHEN the stomach is in a sound state, and digestion is properly performed, the spirits are good, and the body is light and easy; but when that organ is out of order, a languor, debility, melancholy, watchfulness, or troublesome dreams, the night-mare, &c. are the consequences. Grateful food, strong wine, or other spirituous liquors, no sooner touch the stomach of one ready to faint from emptiness, than they communicate new life and strength to the whole body: and, on the other hand, several poisons occasion violent sickness, vomiting, fainting, tremors, convulsions, stupidity, an intermitting pulse, difficult breathing, coldness of the extremities, and other symptoms. A fever, *delirium*, and violent convulsions, have been

* Histoire de l'Acad. royale des sciences, ann. 1717.

† Although in these cases, the changes produced in the body are owing to the passions of the mind; yet as the mind is only affected thro' the intervention of the optic and auditory nerves, they seem proper enough instances of the general sympathy that extends thro' the whole nervous system.

been produced by a pin sticking in the coats of the stomach * : and worms, affecting either this part or the intestines, occasion a surprising variety of symptoms.

EPILEPTIC fits have proceeded from a rough bone or cartilaginous substance irritating the nerves of the great toe, or the calf of the leg ; and the wound of a tendon or nerve has been the cause of a fever, *delirium*, tremors, violent convulsions, a *tetanus*, and death.

MANY more examples might be mentioned, were it necessary, of that general sympathy which prevails throughout the whole body. But there is nothing which sets this matter in so clear a light, as the effects of *opium* : for a solution of this substance injected into the great guts of a dog, in a few minutes brought on a palsy of his posterior extremities, attended with a stupor and convulsions †. Some days after, a like solution being injected, by a perforation through the teguments into the *abdomen* of the same dog, he became almost instantaneously paralytic, and died in a few minutes ‡.

A solution of *opium* injected either into the stomach or intestines of frogs, or even applied to the muscles of their belly laid bare, produces a paralytic weakness, a stupor, and death at last ; altho' such is the nature of these animals, that *opium* does not kill them near soon as it does dogs ||.

FROM these experiments it appears, that not only those nerves, to which *opium* is immediately applied, are rendered incapable of performing their office ; but that the brain, spinal marrow, and whole nervous system are affected in the same manner, *solely* by the action of the *opium* on the nerves which it touches. For its effects upon dogs are too instantaneous to allow of the supposition, that the more subtil parts of this poison are received into the blood, and by that means are conveyed to the brain : and in frogs, after the heart is taken out, and consequently a stop put to the circulation, yet

* Hildan. centur. 2. observ. 34.

† See above, p. 317.

‡ Ibid. p. 318.

|| See above, p. 309. &c.

yet a solution of *opium* injected into the stomach and intestines has the same effect as when these animals are intire *.

II. BESIDES this general consent (10.) which prevails throughout the whole body, there is a particular and very remarkable sympathy between several of its organs, by means of which many operations are carried on in a sound state; and pain, convulsive motions, and other morbid symptoms, are often produced in such parts as have no near connection with those that are immediately affected.

To illustrate this, I shall give several instances, beginning with the head, and taking the parts in their order downwards.

(a) THE HEAD. Violent pains in the head, which have their seat most commonly in the membranes of the brain or *pericranium*, are frequently attended with a sickness at the stomach and vomiting. The *spasmus cynicus*, locking of the jaws, and an universal *tetanus*, have followed a wound of the left side of the head, by which the temporal muscle was divided †. Light and noise are offensive, both to the eyes and ears in severe headaches. Wounds and contusions of the brain generally occasion bilious vomitings. Certain impressions made on the *sensorium commune* by external objects instantly give the eyes either a dull, a lively, or a fierce look.

GRIEF, vexation, or fear, lessen the secretion of the *saliva*, destroy the appetite, and sometimes occasion a looseness. The great consent between the brain and heart appears from the sudden and remarkable effects of the passions on the latter.

(b) THE EYES. When one eye is affected with an inflammation, a cataract, or the *gutta serena*, the other is often soon after attacked with the same disease. The contraction of the pupil is not owing to light acting as a *stimulus* on the *iris*, but solely to the sympathy between this membrane and the *retina* ‡. There is such a sympathy between the two pupils, that, even in a *gutta serena*, the

* See above, p. 197. 309. and 320.

† Hildan. Centur. 5. observ. 9.

‡ See Essay on the vital motions of animals, §. 7.

the pupil of the morbid eye is observed to follow the motions of the sound one. We shut both eye-lids whether we will or no, as often as any thing threatens to hurt either eye. A bright light coming suddenly on the eyes sometimes occasions sneezing. Hippocrates has observed, that the unexpected sight of a serpent will make the countenance pale *. The sight of grateful food occasions an uncommon flux of the *saliva*, in an hungry person. Yawning and vomiting are often catching.

(c) THE EARS. The noise of a file and other harsh sounds affect the teeth with an uneasy sensation. The whetting of a knife has caused the gums to bleed †. Great and unexpected sounds, such as the explosion of a cannon or musket, make us instantly close our eye-lids. As the ear is frequently pained when the *fauces* are inflamed, so an irritation of the *meatus auditorius* will often excite coughing, and sometimes vomiting ‡. A constant pain of one side of the head, with a numbness of the left arm and leg, a suppression of the *menfes*, and epileptic fits, have all been occasioned by a glass-ball, not larger than a pea, sticking in the ear ||.

(d) THE NOSE. The *effluvia* of hungry water, or spirit of wine, drawn strongly into the nostrils, increase the derivation of the salival juice into the mouth, and sometimes stop a tickling cough. The smell of grateful food makes the *saliva* flow when one is hungry. Sternutatories not only increase the secretion from the nose, but also from the lachrymal vessels. After smelling to volatile salts, or eating too much strong mustard with one's meat, a pain is often felt above the eye-brows; and it is observable, that after taking a large draught of cold water in winter, that part of the fore-head immediately above the nose is affected with a painful sensation. Acrid substances applied to the olfactory nerves, bring the diaphragm, intercostal and abdominal muscles, into convulsive motions.

Mr

* Lib. de humoribus.

† Boyle's Usefulness of experimental philos. part 2d, p. 248.

‡ Pechlin, Observat. med. lib. 2. No. 45. || Hildan. Centur. 1. observat. 4.

MR BOYLE mentions several, who were purged by smelling to a cathartic medicine *; and we are told, that, in some, the effect failed, when, from a *coryza*, or obstruction of the membrane of the nose, the olfactory nerves had lost their power of distinguishing smells.

(e) THE TEETH. A rotten tooth will sometimes occasion a violent pain in a sound one, though at a distance from it; and the pain will cease as soon as the spoiled tooth is drawn, or its nerve destroyed. A pain in the teeth often affects the cheek-bone, one side of the head, the throat, and the corresponding ear. Children, from the irritation of the gums in teething, are liable to vomiting, purging, a cough, a fever, and convulsions.

(f) THE TRACHEA. An irritation of the windpipe, or any of its small branches, raises coughing, or a convulsive motion of the muscles employed in expiration; and a *nausea*, vomiting, and convulsions, are sometimes the consequence of a violent or long continued irritation of these parts.

(g) THE LUNGS. THE sympathy of the lungs with the diaphragm and intercostal muscles, is evident from their motion, even in ordinary respiration, but still more so in the laborious breathing which is always the consequence of a difficult passage of the blood through the pulmonary vessels.

(h) THE DIAPHRAGM. When the diaphragm is inflamed, the stomach, brain, and muscles of the face are affected by sympathy, as appears from the *delirium*, vomiting, and *risus Sardonius*, which attend this disease.

(i) THE STOMACH and INTESTINES. A disordered state of the stomach and intestines, with wind or noxious humours lodging in them, will sometimes so affect the brain as to deprive people of their reason. At other times, the same causes will produce a *vertigo*, *cephalæa*, *hemicrania*, *clavus hystericus*, palpitations, intermissions of the pulse, difficulty of breathing, sudden flushings of heat, and sweating, &c. After hard drinking, or a large dose of *opium*, the eyes lose their lustre. The headach, after a debauch, proceeds chiefly from the stomach, as appears by the removal of the pain, upon

R r r
drinking

* Usefulness of Experimental philosophy, part. 2. p. 242.

drinking a few glasses of strong wine. The disorder of the stomach will sometimes occasion dimness of sight *. I know a lady, to whom every object appears as if covered with a thick smoke, as often as her stomach is loaded with an acid; and who, therefore, finds vomits, absorbent powders, and bitters, her best ophthalmic medicines. Another lady, with tender eyes, seldom has any considerable pain or sickness at her stomach, without her head being affected, and her eyelids or eyes becoming, in some degree, inflamed. In little more than half an hour after swallowing fifteen or twenty grains of the *extractum cicutæ*, I have been often affected with a weakness and dazzling of my eyes, together with a giddiness and debility of my whole body, especially the muscles of my legs and arms; so that, when I attempted to walk, I was apt to stagger like a person who had drunk too much strong liquor.

A convulsive motion of the stomach and intestines often spreads to the throat, where it occasions a difficulty of breathing, and a sense of suffocation: On the other hand, an irritation of the *fauces*, or *pharynx*, excites vomiting. A *nausea* or disagreeable sensation in the stomach makes the pulse quicker and smaller, raises a sweat, and sometimes greatly increases the secretion of the *saliva*, or urine. When the stomach is empty, and affected with a sense of hunger, the salival juice flows much more copiously into the mouth than after a full meal, or when the natural appetite for food is wanting. An inflammation of the stomach and bowels is attended, in the beginning, with a shivering of the whole body, and a great coldness of the hands and feet. Long continued vomiting and purging occasion violent cramps of the muscles of the legs and thighs; and the dry belly-ach brings on a palsy of the extremities. A tremor of the hands is often lessened or removed, for a while, by a dram, or some strong wine; and this effect is owing *solely* to the action of these liquors on the stomach, and not to their having entered the blood, which does not happen so soon. The particular sympathy of the stomach with the diaphragm and abdominal

* Lommii Observat. med. lib. 2.

nal muscles, appears from their convulsive motions in vomiting, and in the hiccup. A violent spasmodic pain in the stomach or intestines, often renders the pulse much slower than in a state of health.

AN inflammation of the intestines is frequently attended with vomiting, and a suppression of urine. An *opisthotonus* or a *tetanus* is often occasioned, in hot climates, by a retention of the *meconium*, or other acrid humours, in the bowels of infants. That itching of the nose which is a common sign of worms, seems to indicate a particular sympathy between this part and the intestines; and the many other symptoms produced by worms, which I shall have occasion to mention afterwards, shew a remarkable and extensive consent between the first passages and many other parts of the body.

(k) THE LIVER. Stones irritating the biliary ducts frequently occasion a *nausea* and vomiting. An inflammation of the liver is generally accompanied with a vomiting and the hiccup, and often with a pain between the *vertebræ* of the neck and top of the shoulder. In a suppuration of the liver, I have twice seen the patients affected with a numbness and debility of the right arm, thigh, and leg.

(l) THE KIDNEYS and URETERS. A *nausea*, vomiting, costiveness, and inflation of the bowels, are often produced by an inflammation in the kidneys, or stones in the ureters. A stone in the *pelvis* of the kidney, or in the *ureter*, sometimes occasions a frequent inclination to make urine, and a heat in the extremity of the *urethra*. When one of the kidneys is inflamed, little urine is separated by the other, probably on account of a spasmodic stricture of its secretory vessels. When a stone is passing through the *ureter*, the testicle of the same side is sometimes drawn upwards, and swells; and an erect posture is then painful.

(m) THE BLADDER and RECTUM. An irritation of the neck of the bladder, or extremity of the *rectum*, is the cause of a constant contraction of the diaphragm and abdominal muscles. A strangu-

ry and *tenesmus* mutually occasion each other. The pain of the hæmorrhoids is sometimes accompanied with a sickness at the stomach, and faintishness. A stone or ulcer in the bladder is attended with a sharp pain near the end of the *urethra*, especially after making water. I had, some years since, a patient with an ulcer in the bladder, who, when he passed his urine, had not only a violent pain in the point of the *penis*, but this pain descended down his thighs and legs, and affected the soles of his feet, as if he had been standing barefooted on burning coals.

(n) THE GENITALS in MEN. At the time of puberty, not only the voice, but the whole body undergoes a sensible change, which is probably owing to the *stimulus* communicated to the nerves of the genital parts by the *semen*; for we certainly know that other *stimuli*, applied to the nerves of the nose or stomach, according to their nature, will either instantaneously impart new vigour to the whole body, or soon occasion a general *stupor* and debility. It is owing to a sympathy with the *glans*, that the *vesiculæ seminales* are contracted in time of coition; and, when the membrane which lines the lower part of the *urethra* is stimulated by the *semen*, the *acceleratores urinae* are excited into convulsive motions.

(o) THE UTERUS. The great variety of symptoms in the hysteric disease is the reason why a more extensive sympathy has been ascribed to the womb, than to any other part, except the brain. But, although these symptoms proceed from the womb much less frequently than has been imagined; yet the vomiting which generally accompanies an inflammation of that organ, the *nausea*, and depraved appetite after conception, the violent contraction of the diaphragm and abdominal muscles in delivery, the headach, and the heat and pain in the back and bowels about the time of menstruation, are sufficient proofs of the consent between the *uterus* and several other parts of the body. But there is no part so much affected by the different states of the womb as the breasts, which become more turgid before every appearance of the *menfes*, and subside after the period is over. The changes that happen to the breasts in time of pregnancy, and after delivery, are still more remarkable.

(p) THE

(p) THE EXTREMITIES. Strait shoes give some people a head-ach; while sinapisms applied to the soles of the feet, or blisters to the legs, often lessen, and sometimes remove a *delirium*. In an obstinate costiveness, cold water thrown on the feet and legs has sometimes opened the body, after many other remedies had failed. By tickling the soles of the feet, not only the muscles of the legs, but of the whole body, may be thrown into convulsions. An *opisthotonus*, with convulsions returning every day, has been owing to a wound in the sole of the foot by a nail *; and the *spasmus cynicus*, to a violent pain in the toes †. The locking of the jaws sometimes happens after amputations of the extremities, or lacerations of the nerves or membranes.

MANY other examples of sympathy, both in a sound and morbid state of the body, might be mentioned; but the above will be sufficient at present, as I shall have afterwards frequent occasion to touch on the same subject.

12. ALL sympathy or consent supposes feeling; and therefore must be owing to the nerves, which are the sole instruments of sensation (5). The truth of this seems to be fully evinced, by the following experiment. When the hinder toes of a frog are wounded, immediately after cutting off its head, there is either no motion at all excited in the muscles of the legs, or a very inconsiderable one. But if the toes of this animal be pinched, or wounded with a pen-knife, ten or fifteen minutes after decollation, the muscles, not only of the legs and thighs, but also of the trunk of the body, are, for the most part, strongly convulsed, and the frog sometimes moves from one place to another. In this case, is not the irritation of the toes immediately after decollation rendered ineffectual to produce any motion in the muscles of the legs and thighs, by the greater pain occasioned by cutting off the head? And are not the muscles of the posterior extremities, as well as of the trunk of the body, brought into action by wounding the toes fifteen minutes after decollation, because the pain produced by cutting

* Nova Act. Acad. Cæsar. natur. curios. tom. 1. p. 16.

† Hoffman. System. med. tom. 3. sect. 1. cap. 5. No. 30.

ting off the head is now so much lessened, as not to prevent the animal from feeling very sensibly, when its toes are hurt?

BUT further, that all sympathy is owing to feeling, and consequently proceeds from the nerves, appears evident, because the changes in the body, occasioned by the sympathy of the parts, are stopt by whatever affects the nervous system so strongly as to overcome the sensations that produced those changes. Thus the hiccup is stopt by terror, fear, surprise, or other strong passions. An irritation of the nose will not occasion sneezing, when the first effort to sneeze is attended with an acute pain in some of the muscles of the back or sides from a rheumatic cause. Hungary water, or volatile spirits, drawn strongly into the nose, will often stop a tickling cough; and *laudanum* taken by the mouth, or given in a clyster, by weakening the sentient power of the nerves, will lessen or remove the sympathetic vomiting, arising from a stone in the kidney or *ureter*, and the violent contractions of the diaphragm and abdominal muscles, occasioned by a *tenesmus* or stranguy.

COULD we suppose the circulation of the blood were to remain, after a total abolition of the sentient powers of the brain and nerves, there would be no more sympathy between the parts of such an animal body than between those of any hydraulic machine. As in this case the motion of the fluids would be merely mechanical, so every change made in any of its parts must be the result of mechanism alone, and consequently, wholly different from consent, which, as it depends upon feeling, cannot be explained upon mechanical principles.

13. THOSE sympathies which have been ascribed by some authors to the *cela cellulosa*, blood-vessels, membranes, and the similarity of parts, if duly considered, will appear either to proceed from the nerves, or not to deserve the name of consent or sympathy.

WITH regard to the cellular membrane, as in a natural state it has little or no sensibility, so it must, of all other parts, be the least subject to sympathetic affections. Its cells have every where a free communication; and therefore air, water, purulent matter, or other humours, are often conveyed, by their means, from one part of the

the body to another. But this cannot be properly referred to sympathy, and is no more than what happens to a sponge, a piece of sugar, or other porous substances.

THE system of blood-vessels affords us no more instances of true sympathy than the *tela cellulosa*, except what may be owing to the nerves which belong to these vessels. The changes made in the circulation, and the morbid symptoms produced, or removed by the force of the blood being turned upon different parts of the body, the absorption of venereal contagion, of *pus*, or other humours, and their translation to distant parts, are not, strictly speaking, instances of consent; but are solely the consequences of the circulation of the fluids, and the communication between the several parts of the vascular system. The changes, indeed, produced in the motion of the blood, and in the various secretory organs, by different affections of the mind, are undoubtedly owing to sympathy, not, however, of the blood-vessels, but of the brain and nerves, as will appear afterwards.

THE various instances of consent from the continuity of membranes, are, strictly speaking, owing to the nerves themselves, with which those membranes are supplied; for, were they destitute of nerves and feeling, no such consent could happen. If the heat and pain in the extremity of the *urethra* from a stone or ulcer in the bladder, and the itching in the nose, from worms in the intestines, and such like symptoms, were owing to the continuity of the membranes affected, the gullet and *fauces* ought to suffer more than the nose, and the lower or middle parts of the *urethra* should be more pained than its extremity. Further, if these instances of sympathy were merely owing to the continuity of membranes, why does not the bladder suffer in a *gonorrhœa*, where there is a great irritation and pain near the extremity of the *urethra*?

ALTHO' a deafness has been sometimes cured by purging, this no more argues a sympathy between the ears and intestines, from the continuity of the membrane that lines them*, than the cure of an *ophthalmia* by the same remedy, proves a sympathy between the
eyes

* Haller. Prim. lin. physiolog. § 555.

eyes and bowels. The effect in both cases, is chiefly owing to the derivation of the humours from the parts affected ; and hence blistering the head often affords a more speedy relief in those diseases than purging.

THE sympathy between the breasts and the *uterus*, has been derived from the similarity of their structure, or of the liquors secreted by them. But altho' those parts were much more similar in these respects than they really are, yet if there were no connection between them by means of blood-vessels, or consent by means of nerves, it would be difficult to conceive, how the condition of the one could be so much affected by that of the other ; and much more how a titillation of the one should communicate a particular sensation to the other. The similarity of structure between the muscles of the legs and arms, between the testicles, and between the parotid glands, is considerably greater than between the womb and breasts ; and yet the former have no such sympathy as the latter.

14. ALTHOUGH it may appear, from what has been said, that all real consent between the different parts of the body is owing to the nerves ; yet it will be found very hard to account, particularly, for the various instances of sympathy, either in a sound or morbid state.

THE prevailing opinion has been, that these sympathies are owing to the communications between the nerves, and particularly to the connection which the intercostals have with the fifth, sixth, and eighth pairs, and with almost all those which proceed from the spinal marrow •. Upon this principle it has been thought easy to
trace

• Hippocrates was not ignorant of a general sympathy between the parts of the body ; and Galen treats particularly of those diseases which arise from sympathy or consent : but he was so far from having any notion that sympathetic affections were owing to the nerves, that he ascribes those headaches which do not proceed from any fault in the head, to vapours ascending from the stomach or *uterus*. Succeeding writers, even as far down as Fernelius and Sennerthus seem to do little more than copy what Galen had said on this subject. Andreas Laurentius, who wrote about the year 1600, ascribes the sympathy between the *mammæ* and *uterus*, partly to the intercostal nerve, which sends some branches to the organs of generation, and partly to the *vena azygos*, which terminates in the left spermatic vein. He deduces the vomiting

trace the various sympathies, not only between the several parts of the *abdomen*, but also between them and the head, neck, *thorax*, and extremities. But however plausible this theory may appear at first view, and how readily soever it may seem to explain many remarkable instances of consent, yet a more strict examination will shew it to be liable to insuperable difficulties.

(a) SINCE every individual nerve appears to be quite distinct from every other, not only in its rise from the medullary substance of the brain or spinal marrow, but also in its progress to that part where it terminates (2.), it follows, that the various instances of sympathy, observed between the different parts of the body, cannot be owing to any communication or *anastomosis* of their nerves; and consequently that it can be here of no use minutely to inquire into the numerous connections which the intercostal nerves have with the fifth, sixth, and eighth pairs, and with those of the spinal marrow.

BUT, lest it should be alledged, that the course of the nervous filaments in the *ganglia* is so intricate, that it is not altogether clear, whether they may not intermix or communicate with one another in their passage through those bodies, it will be necessary to offer some less doubtful arguments, for proving that the sympathy of the several parts does not depend on any union or *anastomosis* between their nerves.

(b) IF there were any *anastomosis*, or real communication between the nerves of the same or different trunks, either in the *ganglia* or
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elsewhere,

vomiting in a *nephritis* partly from the nerves, which the kidneys have from the stomachic *plexus*, and partly from their exterior coat, being a continuation of that which covers the bottom of the stomach. Casper Bauchinus derives the consent between the nostrils and exterior parts of the abdomen, from the communication between the epigastric and mammary veins. Riolan, who flourished before the middle of the seventeenth century, has not, with all his learning, made any improvement in the doctrine of sympathy; and his cotemporary Riverius ascribes sympathetic diseases to five causes, *viz.* the connection, situation, vicinity, or similarity of the parts, or to their having the same kind of office. Dr Willis, who has given a more accurate description of the brain and nerves than any anatomist before him, endeavoured, first, to explain the various instances of sympathy between the parts of the body, from the connection or communication of their nerves. This doctrine was afterwards further illustrated by Vieussens, and has been embraced by most of the later writers.

elsewhere, it is natural to think, that a confusion would necessarily happen in our sensations, as well as in the motions of our several muscles; for the impressions of external objects would be communicated, at the places of union, to other nerves than those affected; and the change produced by the will in any nerve, at its origin in the brain or spinal marrow, in order for moving a particular muscle, would affect all those nerves with which it has any communication by means of the *ganglia* or otherwise.

(c) IT does not appear, that there is any sympathy between the nerves that are derived from the same trunk, by means of the membranes that surround them. If the *dura mater* were endowed with that degree of sensibility, and with those powers of oscillation, which have been ascribed to it by several authors, this opinion would not appear altogether improbable; but as the membranes of the brain, and those productions of them which surround the nerves, seem, in a natural state, to be possessed only of a very obtuse kind of feeling *, and are altogether destitute of motion, we have no reason to ascribe the various instances of sympathy between the different parts of the body to their sensibility or moving power.

(d) WE observe a remarkable sympathy between many parts, whose nerves have certainly not the smallest communication with one another. Thus the dimness of sight occasioned by a disorder of the stomach, the *nausea* upon seeing others vomit, and the flux of the *saliva* into the mouth of a hungry person, at the sight of savoury food †, are proofs, that the stomach and salivary glands sympathize with the *retina*, though there is no communication between the optic nerves and any other. A shuddering is excited by particular sounds, and yet the *portio mollis* of the auditory nerve, after it leaves

* The experiments of the learned M. de Haller, although they do not prove these membranes to be wholly insensible, yet they certainly shew them to have no painful or acute feeling in a sound state. Vid. *Acta Gottingen.* vol. 2. and *Physiological Essays*, edit. 2.

† In these instances, the changes in the stomach and salivary glands are produced through the intervention of the brain and sentient principle: for thinking strongly on savoury victuals, or disagreeable medicines, will have almost the same effects on some people as seeing them. But since an impression on the optic nerve can, by means of the brain, occasion vomiting, and an increased discharge of the salivary juice, why may not impressions on the other nerves produce various other sympathies in the same manner? But of this more hereafter.

leaves the brain, does not appear to communicate with the *portio dura*, nor any other nerve. Although the optic nerves unite at the *cella turcica*, yet it has been shown, that their fibres do not cross, intermix, or truly communicate with each other *; nevertheless, there is a considerable sympathy between the two eyes. Although the nerves of the two kidneys do not appear to have any connection with each other, yet, when one of these glands is inflamed, or irritated by a stone, the secretion from the other is frequently much diminished. We know for certain, that the different size of the pupil in different lights, is owing to a consent between the *retina* and *uvea*, and yet the optic nerves, and those belonging to the *uvea*, have no communication in their course from the brain to the eye. Nor can any sympathy be supposed to arise from the nerves of the *uvea*, passing between the *retina* and *tunica choroidea*, as there is no *anastomosis*, nor any other kind of union between them. The nerves, with which the *uvea* of the two eyes are furnished, have no connection; and yet we find a most remarkable sympathy between the motions of the two pupils.

ALMOST the whole muscles of the body may be brought into convulsive motions, by tickling the soles of the feet, or the sides; nay, the dread of this will affect some people. Now, these motions cannot reasonably be deduced from the connection of the intercostal nerves with those of the spine; or, if they could, it would follow that the stomach and bowels should suffer, at least, equally with the diaphragm and muscles of the trunk of the body.

(e) IF the consent between the *viscera* of the *abdomen*, and the other parts of the body, be owing to a communication of nerves, by means of the intercostals; why do not all those parts sympathize, whose nerves are either derived from, or communicate with the intercostals? Why, in the *nephritis*, does the stomach suffer more than the intestines? and why are not the lungs and other parts at all affected in this disease? Why does not an irritation of the bladder from a stone commonly occasion a *nausea* and vomiting; since the bladder, as well as the kidneys, has its nerves partly from the

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eighth

* See Vesal. Anatom. corp. human. lib. 4. cap. 4. ; and Santorin. Observat. anatom. p. 63.

eighth pair and intercostals? Why does an irritation of the nose occasion sneezing only, and not coughing, vomiting, purging, or the hiccup? Why does not a blister, applied from the ear to the top of the shoulder, bring on a convulsive motion, or some other affection of the diaphragm, since the connection of the phrenic nerves with the 2d and 3d cervical pairs is much greater, and less remote than with the nerves of the nose *?

If the *delirium* which generally attends an inflammation of the diaphragm, were owing, as has been alledged, to the remote connection between the phrenic and fifth pair of nerves which sends filaments to the *dura mater*; why should not an inflammation of the lungs, stomach, and intestines be attended with that symptom as often, and in a greater degree, since the fifth pair of nerves hath a more immediate connection with the intercostal than with the phrenic nerves? Why does an irritation of the sphincters of the *anus* or bladder occasion a continued contraction of the diaphragm and abdominal muscles, rather than an alternate motion of those parts, as in coughing and the hiccup, when either the *trachea* or left orifice of the stomach is irritated? Since the diaphragm sympathizes with the nose, lungs, *uterus*, *rectum*, and bladder, why do not these parts suffer equally when that muscle is inflamed, or otherwise violently affected? If the sympathy between the nose and diaphragm be owing, not to the olfactory nerves, but to a branch of the fifth pair sent to the nose, why do not sternutatories excite convulsive motions of the muscles of the face, to which the 5th pair is distributed, as well as spasms of the diaphragm, whose nerves can have only a remote connection with the fifth pair, by means of the

* The pain between the *vertebræ* of the neck and top of the shoulder, which sometimes attends an inflammation of the liver or diaphragm, has been ascribed to the connection between the phrenic nerves and the 2d and 3d cervical pairs: But, if this were the case, why should this symptom be so rarely observed in a *paraphrenitis*, as to be omitted by most authors? and why should not the diaphragm be disturbed in its motions, when the 2d and 3d pairs of cervical nerves are irritated by blisters, the extirpation of tumours, or other causes? It is not easy to say, what may be the cause of that pain in the neck, and top of the shoulder, which often attends a *hepatitis*; but there seems to be good reason to doubt of its proceeding from any connection between the phrenic and 2d and 3d cervical nerves. Some of the older physicians ascribed this symptom to the weight of the inflamed and swelled liver drawing downwards, and stretching the membranes that line the *thorax* (a).

(a) N. Piso de cognoscend. et curand. morb. lib. 3. cap. 25.

the intercostals *; And why does not the great irritation of some of the filaments of the fifth pair of nerves, in the toothach, produce the convulsive motion of sneezing? If the flux of tears occasioned by grief or joy were owing, as Dr Willis alledges, to the communication between the fifth pair of nerves which serves the lachrymal glands, and the intercostals which are distributed to the *præcordia* †; why do not those affections of the mind produce an increased excretion of the salival as well as lachrymal juice? If the disturbance of the motion of the heart, from certain sounds, were owing, as Vieussens imagines, to the seventh and eighth pair of nerves being partly composed of medullary fibres, derived from a particular *fasciculus* arising from the *cerebellum* ‡, why should not the muscles, which are supplied with nerves from the sixth pair and the *portio dura* of the seventh, be equally affected; since their connection, at their origin, with the *portio mollis* is not less than that of the eighth pair ||? Or why, in violent palpitations of the heart, are not the auditory nerves at all affected? The truth is, the changes in the motion of the heart occasioned by external objects, acting either on the organs of sight or hearing, are not owing to any communication of their nerves with those of the heart, but to the impressions made on the *sensorium commune*, and the affections of the mind excited thereby**. If the sympathy between the different parts be owing to their nerves being derived from the same trunk, why is there not the same consent between the several muscles of the foot, of the leg, and of the thigh, as between the kidneys and the stomach, or between the nose and the diaphragm? Lastly, if an irritation of the alimentary canal in hysteric women, sometimes produces convulsions of the legs, by reason of the communication between the intercostals and the two last lumbar nerves; why are not the stomach and bowels seized with spasms or convulsive

* When sneezing is stopt, by pressing the nose with one's finger and thumb, near the internal angle of each eye; this effect is not owing, as some have thought, to the pressure made on the nerves sent to the nose from the ophthalmic branch of the fifth pair; for sneezing may be stopt almost as readily by pressing hard with one's fingers on the forehead. In both cases sneezing is prevented in the same manner, *viz.* by the uneasy sensation occasioned by strongly pressing on the brow or nose.

† Anatom. cerebri, edit. in 4to, p. 288.

‡ Neurograph. lib. 3. cap. 4. || Id. lib. 1. cap. 12. ** See below, No. 17.]

five motions, from the violent pain of the gout in the knees, ancles, or feet? These questions, I doubt, will scarce admit of a satisfactory answer, upon the principle of sympathy depending on the communication or connection of nerves.

15. IF, therefore, the various instances of sympathy cannot be accounted for from any union or *anastomosis* of the nerves, in their way from the brain to the several organs; and if there are many remarkable instances of consent between parts whose nerves have no connection at all; it follows, that all sympathy must be referred to the brain itself and spinal marrow, the source of all the nerves.

BUT for a more direct proof of this, we may observe, that the consent of the several parts instantly ceases, when their communication with the origin of the nerves is interrupted. Thus, though the muscular coat of the stomach, in an animal newly dead, is excited into contraction by irritation, yet the diaphragm is no ways affected by his *stimulus*. In like manner, when any of the muscles of the leg of a frog are pricked, most of the muscles of the legs and thighs contract, even after cutting off its head, if the spinal marrow be left entire; but when that is destroyed, although the fibres of the stimulated muscle are affected with a weak tremulous motion, yet the neighbouring muscles remain wholly at rest.

FURTHER, the effects of pain, and of fear and other passions, in preventing several sympathetic motions, seem to shew, that the cause of that consent which obtains between the parts of animals is to be referred to the origin of the nerves: and, since certain affections of the mind, excited by the action of external objects on the organs of sense, produce extraordinary motions and other effects in the body, merely by affecting the brain; why may not impressions made on the nerves in other parts produce likewise, through the intervention of the brain, various motions and other effects in distant parts of the body? The analogy is obvious.

LASTLY, Notwithstanding the many sympathetic motions, which are daily observed by physicians to arise from an irritation of the nerves in different parts of the body; yet, when the nerve going to any muscle is irritated, there is no motion excited in any part, except

cept in the muscle to which it is distributed *. Does it not hence appear highly probable, that the various sympathetic motions of animals produced by irritation, whether in a sound or morbid state, are owing, not to any union or connection of their nerves, but to particular sensations excited in certain organs, and thence communicated to the brain or spinal marrow? For, if this were not the case, why should not the diaphragm, for example, be convulsed, by irritating the nerves that go to the bladder and *intestinum rectum*, as well as when these parts themselves are affected by an unusual *stimulus*?

IF the sympathies observed between the different parts of the body be owing to particular sensations excited in them, and thence communicated to the brain; we may easily see why an irritation of the *intestinum jejunum* does not affect the diaphragm so much as an irritation of the *rectum*; for though the *jejunum* is not less sensible than the *rectum*, and the nerves of both have the same remote connection with the nerves of the diaphragm; yet the sensations excited by the same *stimuli*, acting on the *jejunum* and *rectum*, are very different, and therefore must affect the brain or common *sensorium* differently. An irritation of the nerves of the face does not produce any such convulsive motion of the muscles of respiration, as happens in sneezing; because it does not occasion that kind of sensation which is excited by *stimuli* applied to the nose: And the diaphragm, which is brought into a continued contraction, when the extremity of the *rectum* or neck of the bladder is painfully affected, is agitated with alternate convulsions, when the left orifice of the stomach is irritated, because very different sensations are excited by an irritation of those parts.

FURTHER, when the *meatus auditorius* is irritated, by introducing into it a feather, or any such substance, an inclination to cough is often excited, especially if the membrane of the *trachea* has been rendered more sensible than usual, by catching cold; but, when the *meatus auditorius* is violently pained, in consequence of an inflammation in it, no coughing is occasioned: From which it follows, that the sympathy between that *meatus* and the organs of respiration in the former case, cannot be owing to any connection between

* Memoires sur la nature sensible et irritable, tom. 1. p. 237.

tween their nerves, or indeed to any mechanical cause, but proceeds from a particular feeling, and must be referred to the *sensorium commune*. In like manner, neither an acrid injection of a solution of the corrosive sublimate in water, nor the introduction of a *catheter* into the *urethra*, occasions any alternate convulsive motions of the *musculi acceleratores urinæ*; although the *semen*, which stimulates the nerves of the *urethra* much more gently, produces this effect. Lastly, on this head, although when the sides or soles of the feet are tickled, the body is often thrown into convulsive motions; yet nothing of this kind happens when these parts are either inflamed or wounded; from which it evidently follows, that those motions are occasioned by the particular sensation excited by the tickling, and do not proceed from any sympathy which the nerves of the sides or soles of the feet have with those of the other parts of the body, in consequence of any connection between them.

16. BUT altho', from what has been said, it may appear probable, that all nervous consent proceeds from the brain; yet we cannot pretend, from this principle, to account, in a satisfactory manner, for all the various instances of sympathy observable in the bodies of animals, since many of them may depend on such a state of the brain and other parts as cannot be the object of our senses *.

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* If it should be objected, that it is as difficult to account for a sympathy between the nerves at their origin in the brain, as in their course to the several parts, where they happen to be connected; I answer, that the purpose of these observations is not to explain how the different parts of the body can be endowed, by means of the nerves, either with a sentient or a sympathetic power; but, to endeavour to trace the sympathy of the nerves to its true source, which I take to be the brain and spinal marrow. It would be in vain to inquire further into this matter, unless we knew the minute structure and connections of the several parts of the brain, and were better acquainted with the laws of union between the body and soul, to whose sentient power the sympathy of the nerves, at their origin, must be at last referred: For, if consent supposes feeling (12.), and if feeling cannot, any more than intelligence, be a property of matter however modified; it must follow, that sympathy depends upon a principle that is not mechanical, and that, to suppose it may be owing solely to the particular situation, arrangement, or connection of the medullary fibres of the brain, or to the union of the nerves proceeding from it, is as unreasonable, as to imagine that thought may be the result of a motion among the particles of the animal spirits, or other subtile matter in the brain.

THE sympathy between every individual nerve and the whole system *, will be readily allowed to be owing to the mediation of the brain, and not to any connection or communication among the nerves proceeding from it: I shall, however, mention one experiment as the most decisive of this question.

A solution of *opium*, applied to the abdominal muscles of a frog, whose brain and spinal marrow had been destroyed, did not stop the motion of the heart near so soon as it would have done, if the brain and spinal marrow had been entire †. A clear proof, that the power of *opium*, to destroy the motions of those parts which it does not touch, is owing solely to the mediation of the brain and spinal marrow, and not to any other communication among the nerves.

'Tis true, when a frog is deprived of the brain and spinal marrow, upon applying a solution of *opium* to the abdominal muscles, its heart will cease from motion somewhat sooner than it would otherwise do; but this effect is not to be ascribed to the action of the *opium* on the nerves which it touches, but to some of its finer parts being taken up by the absorbent veins ‡, and carried with the blood to the heart.

THAT life and vigour, which is almost instantaneously communicated to the whole body, by volatile spirits applied to the nose, or cordial medicines received into the stomach, are, like the effects of *opium*, to be referred to some *stimulus* or impression communicated to the brain, by the nerves of the nose and stomach. A dram of brandy acts in the same manner, when it settles a shaking of the hands; and as those epileptic fits which are occasioned by some extraordinary irritation of the nerves of the arms, legs, or toes, do not begin, till after a certain sensation has been propagated from the part irritated to the head; we may safely conclude, that these sympathetic motions proceed from the brain, and not from any connection which the nerves of the parts affected can have, by means

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* See No. 10. above.

† See above, p. 310. 313. 321.

‡ Ibid. p. 321. and 322.

of the intercostals, with the other nerves of the body *. Nor is it more surprising, that an uncommon irritation of any sensible part should, especially in those of a delicate frame, produce convulsive motions of almost the whole body, through the intervention of the brain, than that *opium* applied to the nerves of the stomach, intestines, or abdominal muscles, should quickly destroy the powers of feeling and motion throughout the whole nervous system †.

17. NOTHING makes more sudden or more surprising changes in the body, than the several passions of the mind. These, however, act solely by the mediation of the brain, and, in a strong light, shew its sympathy with every part of the system.

SUCH is the constitution of the animal frame, that certain ideas or affections excited in the mind are always accompanied with corresponding motions or feelings in the body; and these are owing to some change made in the brain and nerves by the mind or sentient principle ‡: but what that change is, or how it produces those effects, we know not: as little can we tell, why shame should raise a heat and redness in the face, while fear is attended with a paleness. These, and many other effects of the different passions, must be referred to the original constitution of our frame, or the laws of union between the soul and body.

BUT although, in these matters, we must confess our ignorance, yet, from what we certainly know of the action of the nerves, we can easily see, that a change in them may occasion many of those effects which are produced by the passions.

As

* Dr Hillary has remarked in the *colica pictonum*, that when the pain in the bowels has continued long, and at last begins to abate, a pain in the shoulder-points, and adjoining muscles, comes on, with an unusual sensation and tingling along the spinal marrow, that soon extends itself from thence to the nerves of the arms and legs; which members first become weak, and afterwards quite paralytic. *Vid.* Hillary on the Epidemical diseases of Barbadoes, p. 184. and 185. Does not this observation seem to shew, that the palsy of the extremities, occasioned by the *colica pictonum*, is not owing to any communication between the nerves of the bowels and of those parts, but proceeds from the spinal marrow, which is first affected?

† See above, p. 321.

‡ By the sentient principle, I understand the mind or soul in man, and that principle in brutes which resembles it. See above, p. 145. &c.

As the force of the heart, and the regularity with which it contracts, depend, in a great measure, on the state of its nerves, so does the action of the arterial system in carrying on the circulation; and particularly those alternate contractions with which the minuter vessels are continually agitated, and to which the motion of the fluids in them is, in a great measure, owing *.

THE other muscles of the body are often, by an uncommon exertion of the nervous power, affected either with alternate convulsive motions, or a continued spasm. It is reasonable, therefore, to think, that the heart and vascular system may suffer in the same manner; and that, when the influence of the nerves is much weakened, or in some measure suspended, the vessels will be relaxed, the circulation will become languid, and an universal debility will ensue.

THE increased force of the heart, and sometimes indeed of the whole muscles of the body, from great anger or rage, is to be ascribed to a stronger exertion of the nervous power; while the trembling and debility produced by fear, arise from a contrary cause.

THE palpitation of the heart from terror seems to proceed from the blood returning to it in too great a quantity, in consequence of a sudden spasm or contraction of the veins. It is also in part occasioned by the heart being rendered more irritable, or being otherwise disturbed by the violent agitation of the nervous system.

THE redness and glow of the face from a sense of shame, are

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most

* It has been shewn, from a variety of facts, as well as from analogy, (see above, p. 228. &c.), that the very small vessels, to which the direct force of the heart does not seem to reach, are endowed with a power of motion excited by the *stimulus* of the fluids, as they pass along; and that these vibratory or oscillatory motions of those vessels are much increased, when they are more than ordinarily irritated, or when, through strong passions or other causes, the nerves are greatly affected.

The speedy inflammation of the eyes, by acrid substances, the inflammation of the skin by blisters and sinapisms, and the increased secretion from the nose and salivary glands, when stimulating substances are taken into the mouth, or applied to the nostrils, can only be accounted for from an increased motion of the small vessels of those parts. And that the circulation of the fluids, in the very small vessels, depends greatly on some influence communicated to them by the nerves, appears from Dr Nuck's having observed the secretion by the glands to be much diminished, or entirely stopt, after their nerves were obstructed or compressed (a).

(a) Adenograph. curios. p. 16.

most probably owing to an increased motion of the small arteries of that part; for the florid colour and sudden warmth seem to be more the consequences of a quicker motion of the blood in these vessels, than a stagnation of it from any compression or spasm of the veins, which would produce but a livid redness and less heat. Besides, we know, that a greater degree of redness is instantly brought on the eyes, and, in a short time, on the skin, by an increased motion of their small vessels upon the application of acrid substances to them.

SOME grow pale upon anger; which effect may be owing to a spasm, or continued contraction of the small arteries of the face, by which the motion of the blood in them will be retarded.

THE paleness from fear may arise from a different cause, *viz.* a deficiency of the nervous power: Hence, though the small vessels are not affected with any spasm, as in anger; yet they are, in a great measure, deprived of their alternate contractions, to which the motion of the blood in them is principally owing. But the more than usual flow of the blood towards the heart, occasioned by terror, seems to shew, that the veins, at least, are suddenly contracted.

THE diminution of perspiration attending such passions as affect us with sadness, may be owing to the impaired force of the heart and arteries: and the *diarrhœa* from fear, may be a consequence of obstructed perspiration, or of that debility and relaxation which fear, or grief, is observed to bring on the alimentary canal.

THE increased secretion of tears from grief, and the great flux of limpid urine, which is often occasioned by fear or vexation, are owing to an increased motion, excited by these passions, in the small arteries and excretory ducts of the lachrymal glands and kidneys.

THE dull look of the eyes in grief, and their lively appearance from joy, depend upon a diminution or increase of the motion of the fluids through the small vessels of that organ, particularly of the *cornea*, in consequence of their vibratory motions being lessened

fened or augmented, by the change which those different passions produce in their nerves.

It would be easy, upon the same principles, to account for various other effects produced by the passions; but what is already said will be sufficient for shewing in what manner we can reason upon this subject.

18. BECAUSE the nerves are observed, in many parts of the body to surround the arteries and veins like small cords, it has been thought, that the sudden changes in the motion of the fluids made by the passions, are owing to these vessels being contracted by such ligatures. But this opinion, though supported by authors of great character *, will, upon a further inquiry, appear inconsistent with what we know for certain of the nature and use of the nerves.

EVERY part endowed with a power of contraction, owes that action either to its muscular structure, or to its elasticity; but as the nerves are in no sense muscular, so they have been proved to be among the least elastic parts of the body. Further, in a natural state, the nerves lie pretty loose in that cellular substance which surrounds the arteries, and are never on the stretch: and, upon making the experiment, we shall find, that the trunks of those nervous branches that encompass the large arteries and veins must be considerably pulled before these vessels can be sensibly contracted. There is no example of any motion being performed by a contraction of the nerves, whose action does not consist in pulling, or in growing more tense at one time than another, but in supplying the muscular fibres with that influence or power which seems to be immediately necessary for their contraction.

LASTLY, it appears from experiments, that the nerves are utterly incapable of any such contraction as is here supposed. Nothing occasions a more sudden or stronger exertion of the nervous power than an irritation of the brain, spinal marrow, or nerves; as appears from the violent convulsions in the muscles and muscular organs, when those parts are injured: but, on such occasions, it has never been observed, that the nerves themselves became shorter, or
underwent

* Willis, Vieussens, &c.

underwent any sensible change. Nay, the illustrious M. de Haller has, after many experiments, justly concluded, that the nerves are not endowed with irritability, or a power of contraction, when stimulated *.

BUT, supposing the nervous filaments could, like cords, straiten the blood-vessels, as several writers have imagined; yet, upon reflection, we shall be convinced, that the changes produced in the body by the several passions cannot be explained upon that principle.

THUS, the redness and glow of the face attending a consciousness of shame, cannot be owing to a constriction of the temporal or jugular veins by the nervous cords surrounding them †; for this would not raise a florid colour, but a redness of a different kind, and accompanied but with little heat.

IN like manner, a compression of the veins of the *penis* by the nerves, will not account for its erection ‡, which is owing more to an increased motion of the blood in the arteries than to any obstruction of its veins ||. Nor is it less credible, that the small arteries of the *penis* should, in consequence of an affection of the mind, be agitated with an uncommon motion, than that the smell, sight, or even remembrance of grateful food, should affect the salivary vessels of a hungry person in a similar manner.

A convulsive contraction of the *plexus renalis* occasioned by fear, might render the urine limpid, by straitening the secretory vessels of the kidneys; but, upon the same principle, it ought also to lessen its quantity, contrary to what happens.

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* It may be proper here to take notice, that although M. de Haller had embraced the doctrine of the nervous *lacquei*, and said more in support of it than any other author; yet he has candidly given up this opinion, upon finding it not confirmed by any of those numerous experiments he has made on living animals. Vid. *Memoires sur la nature sensible et irritable*, tom. 1. p. 238. and 239.

† Vieussens *Neurograph.* lib. 3. cap. 4.

‡ Duvernoy in *Aët. Pretropol.* tom. 2. p. 379. 383. 384.

|| See an Essay on the vital motions, § vi.; and the celebrated Albinus's *Annotationes academicae*, lib. 2. cap. 18.

I shall only add, that it may appear from what has been said, that such expressions as the increased motions, convulsions, or spasmodic contractions of the nerves, are all improper, although they have been frequently used by many learned writers.

19. THERE are many of the most remarkable sympathetic motions, both in a sound and diseased state, in which we can plainly perceive a wise intention. Thus, the contraction of the pupil when light offends the eyes, and of the eye-lids when grosser bodies threaten to hurt them; the vomiting, from a stone in the kidneys and ureters; the coughing occasioned by an irritation of the *meatus auditorius*; the continued contraction of the abdominal muscles and diaphragm in a *tenesmus*, a strangury, and during the pains in labour; the alternate contractions of the same muscles in sneezing, coughing, and in the hiccup; the increased motion of the organs of respiration in the fit of an asthma; the copious secretion of tears and the *saliva*, when stimulating substances are applied to the eyes, or taken into the mouth; and the uncommon flux of humours to every part that is irritated: All these, and many more, are the efforts of nature to free the body of something hurtful; and are so many instances of that principle of self-preservation so conspicuous in all animals. These motions, therefore, cannot, in my opinion, be referred to any connection or communication among the nerves, but to the brain itself, and to that sentient being which animates our whole frame, and which endeavours, at all times, to free the body from whatever occasions pain or uneasiness.

INDEED, when these efforts are unable to expel the offending cause, as in great inflammations of the stomach, or when a large stone is lodged in the kidneys or bladder, they often become hurtful, and increase the pain they were intended to remove. Nay, as in many other instances, the best things may, by excess, become the worst; so this endeavour to free the body, or any of its parts, from what is noxious, is sometimes so strong and impetuous as to have fatal consequences. But, in general, this principle of preservation is highly useful, since without it we should often cherish within our bodies such causes as would, sooner or later, end in our ruin.

NOR

NOR can we consider the mind as acting either ignorantly or perversely, when it sometimes excites such motions in the body as increase its own pain, and, in the end, prove more hurtful than beneficial; for these motions do not proceed, as the followers of Stahl have imagined, from any rational views in the mind, or a consciousness that the welfare of the body demands them, but are an immediate consequence of the disagreeable perception which excites it into action*.

THERE are various instances of sympathy, which seem to be chiefly occasioned by the vicinity of the parts †. Of this kind is, perhaps, the consent between the neck of the bladder and extremity of the *rectum*; whence a violent *tenesmus* and strangury mutually excite each other. The vomiting occasioned by an inflammation of the liver; the pain, swelling, and inflammation of the hand and arm, from a *paronychia*; the increased sensibility of the *retina*, from an inflammation of the *conjunctiva* or *cornea*; the pain and swelling of the face from the toothach, and the pain in the ear from an inflammation of the back part of the *fauces*; the suppression of urine from an inflammation of the intestines or mesentery, or from a severe nephritic paroxysm in either kidney; the sympathy between the *larynx* and *pharynx*, and several others, may be in a great measure owing to the same cause.

To this head also may be referred those sympathies which are sometimes occasioned by hard tumours pressing upon, or irritating the

* See this point further illustrated above, p. 149. 152. 161. 162.

† Parts may suffer from vicinity, although their nerves have no particular sympathy with one another. Thus, pain causes inflammation, not only in the vessels immediately affected, but also in those contiguous to them. Further, any considerable obstruction, though attended with little or no inflammation, may, in some cases, occasion sympathetic affections in the neighbouring parts, by changing the distribution of the blood through the vessels of those parts.

When one of the fingers is inflamed, in consequence of a wound below the nail, or some acrid matter lodged there, the hand, and sometimes the arm, may become swelled and inflamed, not only by means of the pain, which occasions a greater derivation of fluids to the vessels of the finger and hand, but also from a kind of inflammation being propagated up the arm, along the coats of the nerves which are distributed to the fingers.

the nerves that are contiguous to them. Thus, a hard swelling on one side of the neck has occasioned an uneasy sensation near the end of the *radius*, a little above the wrist: and the swelling and drawing up of the testicle, from a stone descending thro' the *ureter*, is probably owing to an irritation of the nerves of the testicle, where they run along the *psoas* muscle, over which the *ureter* passes. But it may be proper to observe, that the heaviness of the eyes, and sleepiness after a full meal, drinking largely of strong liquors, or a dose of *opium*, which have been ascribed to the compression of the third pair of nerves, by the distension of a branch of the carotid artery, which passes over them near their origin, are owing solely to the change produced in the nerves of the stomach; whence the sensibility of the whole system is impaired.

MAY not the complaints of the stomach and bowels, from a suppression of the *menfes*, and soon after conception, be owing not only to a particular sympathy between their nerves, but partly also to the change made in the quantity of the blood thrown upon these parts, by the obstruction of the uterine vessels? And does not the sudden relief, obtained by a small evacuation of blood from the hæmorrhoidal veins, shew, that many disorders may be either occasioned or cured by a small change made in the distribution of the blood to the different parts of the body.

THE pain in the head, sometimes the consequence of wearing strait shoes, is, perhaps, rather to be ascribed to a greater determination of blood to the vessels of the *pericranium*, than to any particular sympathy between the nerves of that part and the feet. And the effect of sinapisms applied to the soles of the feet, in lessening a *delirium*, is chiefly owing to the pain they excite; which, by affecting the whole nervous system, lessens the perception of that irritation in the brain, or its membranes, which is the cause of the *delirium*: and hence it is that sinapisms applied to the hams, or other sensible parts, have produced the same effects as when laid to the feet.

22. LASTLY, In morbid cases, we meet with a variety of anomalous sympathies, which we can neither explain from the vicinity

of the parts, the connection or communication between their nerves, nor from that general tendency to the welfare and preservation of the body, which is so observable in many sympathetic motions that take place in a sound as well as a morbid state.

Of this kind are the purging from smelling to a cathartic medicine; that pungent sensation felt on the top of the left shoulder-blade, when a pimple, a little below the out-side of the right knee, was scratched *; that burning pain which, upon making water, has been felt in the soles of the feet by a person affected with an ulcer in the bladder; the *spasmus cynicus* from a wound in the foot, and the locking of the jaws after an amputation. Thus, what reason can be given why sometimes, after cutting off an arm or leg, those muscles which raise the lower jaw should be affected with a spasm rather than any other muscles? I shall allow, that some symptom of this kind might be expected from the irritation of the nerves of the stump, or from some acrid humour absorbed by the vessels of the fore, and carried to the brain; but in either case, why do the temporal and masseter muscles only suffer?

I think it most probable, that the anomalous sympathies above mentioned, and many others whose cause appears equally obscure, proceed from that general sympathy which prevails thro' the whole nervous system, and which, in certain cases, in consequence of the uncommon weakness or delicacy of a particular organ, makes it suffer, altho' the other parts of the body are not sensibly affected. The following cases, compared together, will serve to illustrate this.

A middle aged woman, who had sprained her right foot and ankle, some weeks after, not only complained of a pain and stiffness in these parts, but also felt, tho' in a much less degree, a tension and soreness over her whole body. On the other hand, a girl of nine years of age, as often as one of her feet was extended so as to bring it nearly to a right line with the leg, and consequently greatly to stretch its ligaments and tendons, was instantly seized with a most violent

* See Hales's Statical essays, vol. 2. p. 60.

violent convulsive cough, which continued without intermission, as long as the foot was kept in that position.

IN the former case, it will readily be allowed, that the stiffness and foreness felt thro' the whole body proceeded from that general sympathy which obtains between all its parts, by the mediation of the brain, which, however, would not have produced such an effect, but for the peculiar delicacy of the nervous system in that patient.

IN the latter case, the convulsive cough occasioned by extending the foot could not proceed from any particular sympathy between this part and the lungs, in consequence of any connection or communication between their nerves, since the nerves of many other parts have an equal or greater connection with those which serve the feet. This convulsive cough, therefore, must be ascribed to a peculiar delicacy or uncommon sensibility of the lungs; whence, in consequence of that general sympathy which prevails thro' the whole nervous system, they were affected with a disagreeable sensation as often as the ligaments and tendons of the ankle and foot were overstretched; which, however, produced no uneasiness nor sympathetic motion in the other parts of the body, because they were endued with no such morbid delicacy or uncommon sensibility.

As a further proof of this, I knew a woman who had so delicate a stomach, that, when this organ was more than usually indisposed, was apt to fall a retching as often as she made the necessary effort to pass water; and I have had several patients affected, in consequence of a virulent *gonorrhœa*, with a gleet and a tenderness, and some degree of foreness in the *urethra*, who, as often as they drank two or three glasses of wine, immediately felt an uncommon uneasiness in that part. This extraordinary sympathy, however, between the stomach and *urethra*, ceased as soon as the latter became quite sound.

SINCE we observe that only those whose nervous system is remarkably delicate are affected with general and violent convulsive motions or spasms from the passions of the mind, disorders in the

primæ viæ, and other causes; have we not reason to conclude, when, in consequence of an irritation of any one part, an uncommon sympathetic motion is produced in a distant organ, with which it has less connection, either by the nerves or blood-vessels, than with many other parts which are noways disturbed, that such sympathetic motion is owing to a peculiar delicacy or mobility of that organ; and that, were the other moving organs of the body equally delicate and sensible, universal or at least more general convulsions or spasms would have been the consequence?

BUT, supposing we could neither explain satisfactorily, nor even conjecture with probability, concerning the cause of many uncommon and anomalous sympathies, it would be no more than what happens to us every day in our inquiries into the more abstruse operations of nature. In every part, even of the inanimate world, we find inexplicable difficulties: what wonder then, if, in the human body, a system so curious, so subtle and compounded, we should meet with many appearances which we cannot at all account for? The farther we push our inquiries into nature, the more shall we be convinced of our ignorance, and how small a portion is known of the works of the Great CREATOR!

“SCARCELY do we guess aright at the things that are upon earth, and with labour do we find the things that are before us *.”

C H A P.

* Wisdom, chap. ix. ver. 16.

C H A P. II.

Of NERVOUS, HYPOCHONDRIAC, and HYSTERIC Disorders
in general.

THE nerves, like the other parts of the body, are liable to various diseases, which may arise from a fault either in their coats, their medullary substance, or in the brain and spinal marrow, from which they all proceed.

THE coats of the nerves may be obstructed, or inflamed, compressed by hard swellings, or irritated by acrid humours. With regard to their medullary substance; if a single nervous filament, exclusive of the membranes surrounding it, be an extremely small canal, we may conceive it, according to the different states of the body, to be endowed with different degrees of firmness or laxity, whence the action of the nerves may perhaps be considerably affected.

THIS nervous canal may likewise be obstructed, tho' such obstruction is rather more likely to arise from some external cause than from any swelling in the medullary substance of which its sides consist, or from the viscidness of the fluid it contains. In the small arterial vessels, obstructions may often happen from a spasm; but altho' the nerves communicate a power of motion to other parts, yet it does not appear that they themselves have any motion.

IF the medullary part of the nerves be simple and not made of vessels, like the other parts of the body, it can neither be liable to obstructions nor inflammations, but may suffer greatly from the irritation of acrid substances.

WITH respect to that fluid which the nerves are supposed to contain, as we are wholly ignorant of its nature, both in a sound and morbid state, we can never know when the diseases of the nerves
arise.

arise from a fault in this fluid, altho' their action must be considerably affected whenever it is vitiated.

WHEN the brain or spinal marrow is obstructed, compressed, irritated, or otherwise diseased, the nerves will suffer almost equally as if they themselves were primarily affected.

IT would be of little use to insist further on those faults in the brain or nerves which may produce diseases, since the subtilty of these parts makes it often impossible for us, either before or after death, to discover precisely from what cause such diseases proceed; nor have we any signs to distinguish from one another those morbid symptoms which may arise from a fault in the coats, the medullary substance, or the fluid of the nerves. But how much soever we may be in the dark about the immediate causes of the diseases of the nerves, yet their effects may all be reduced to some change in that sensibility or moving power which the nerves communicate to the different parts of the body.

THE sentient power of the nerves may be either too acute obtuse, depraved, or wholly wanting; and that power in them which is necessary for muscular motion may be either weakened or quite destroyed.

I. (a) WHEN the feeling of the nerves is too acute; disagreeable or painful sensations and violent or irregular motions will be excited in the body, by the application of such substances to the nerves of the different organs, as in a more healthy and firmer state would either occasion less uneasiness and disturbance, or none at all. In such a condition of the nervous system, the passions of the mind, errors in diet, and changes of heat and cold, or of the weight and humidity of the atmosphere, will be apt to produce morbid symptoms; so that there will be no firm or long continued state of health, but almost a constant succession of greater or less complaints.

(b) ON the other hand, when either the whole nerves, or a part of them, are deprived of a proper degree of sensibility, altho' the body
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in general will then be less apt to be affected by the causes above mentioned; yet, as some of its organs will not be sufficiently irritated by the *stimuli* designed by nature to excite them into action, the action of those parts will be imperfect. Thus, when the nerves of the intestines are less disposed than usual to be affected by their natural *stimuli*, the irritation of the aliments, air, and bile, will only be able to raise a languid peristaltic motion; and therefore the person will become costive. When the sensibility of the *retina* is impaired, objects are seen less distinctly; and when the auditory nerves lose some part of their exquisite sensibility, the ear cannot accurately distinguish the various musical sounds.

(c) WHEN the feeling of the nerves in any of the organs of the body becomes unnatural or depraved, the most disagreeable sensations and alarming symptoms are sometimes raised by the application of such substances as in a sound state would produce no manner of disturbance: And hence we may understand the surprising effects of certain smells, aliments, and medicines, on many delicate people.

THIS uncommon or depraved feeling of the nerves does not always consist in a more acute sensibility; for water will raise violent convulsions in a *hydrophobia*, whilst the *fauces* and *oesophagus* are not at all affected in that manner by solid food; and a small quantity of honey will sometimes occasion more violent gripes than many of the stronger purgatives.

(d) WHEN any of the nerves lose their power of feeling entirely, the organs, or parts to which they are distributed, become quite insensible. When the whole nerves of the organs of sense and voluntary motion are thus affected, whilst the heart and muscles of respiration continue to act, we call the disease an Apoplexy.

2. (a) A greater degree of that power in the nerves which is necessary for motion, can only give more force and steadiness to the muscles, when they all possess it in an equal degree; the increase, therefore,

therefore, of this power is hardly to be accounted a distemper : It is never exerted, except in consequence of an effort of the will, of some affection of the mind, or of the action of some *stimulus* on the brain or nerves ; to the two last of which are to be ascribed all the depraved and irregular motions observed in the body, and not to any real depravation of the nervous power itself, which seems only to occasion diseases when it is either weakened or wholly destroyed. Thus a *tetanus*, or unusual spasmodic contraction of any muscle, is not owing to an increase of that power in its nerves which is necessary for muscular motion, but to an extraordinary exertion of it, in consequence of some uncommon irritation or affection of the brain and nerves.

(b) A diminution of the moving power of the nerves produces a debility of the whole body.

(c) A total want of this power occasions either a partial or universal palsy, according as only a few of the nerves or the whole system is affected. When any of the muscles are deprived of the nervous influence, they are not only rendered paralytic, but soon after become smaller ; because the circulation of the fluids cannot be carried on, as usual, thro' the very small vessels when they are deprived of the nervous power*.

BUT here it will be proper to observe, that, as there is scarce any part of the body without nerves, and very few altogether without feeling, the nerves must not only suffer when they themselves, or the brain and spinal marrow, are primarily affected, but also when the other parts are diseased : and hence the difficulty, perhaps the impossibility, of fixing a certain criterion by which nervous disorders may be distinguished from all others.

ALL diseases may, in some sense, be called affections of the nervous system, because, in almost every disease, the nerves are more or less hurt ; and, in consequence of this, various sensations, motions, and changes, are produced in the body. However, those disorders

* See above, chap. I. No. 8.

disorders may peculiarly deserve the name of NERVOUS, which, on account of an unusual delicacy, or unnatural state of the nerves, are produced by causes which, in people of a sound constitution, would either have no such effects, or at least in a much less degree.

To illustrate this by a few examples. We do not call the tooth-ach a nervous disease, because the nerves of the teeth are greatly pained; but if, from a particular delicacy of constitution, the patient is by this pain thrown into convulsions and faintings, we call these symptoms *nervous*. An obstruction in the coats of the stomach, or other hypochondriac *viscera*, is not, strictly speaking, a nervous disease; but if the nerves of these parts are so changed from their natural state, that low spirits, melancholy, or madness, are the consequence of this obstruction, then these symptoms deserve the name of *nervous*. Again, altho' the fever excited by the painful inflammation of the finger in a *paronychia*, and the fever and vomiting occasioned by a *nephritis*, arise from the sympathy of the nerves; yet such symptoms are not commonly accounted nervous disorders, because they do not indicate any particular unsound state of the nerves, and happen, in some degree, to every one labouring under a *paronychia* or *nephritis*; but if convulsions or faintings are added, then these last symptoms, being the effects of an uncommon delicacy of the nervous system, may be justly called *nervous*. In like manner, the convulsions sometimes preceeding the eruption of the small-pox deserve this name, because they only seize those whose nervous system is easily moved, while the quick pulse, and other feverish symptoms, though excited by the variolous matter acting as a *stimulus* on the nerves, are not reckoned nervous. To conclude, even a *gutta serena*, from a tumor pressing upon the optic nerve, is not, in our sense, so much a nervous disease, as that dimness of sight which is sometimes occasioned by a disorder of the stomach; for the cause now mentioned will produce the *gutta serena* in every person equally; whereas this dimness will only happen to such as have a peculiar delicacy of nerves.

IN treating, therefore, of nervous disorders, I shall confine myself

self chiefly to those complaints which proceed, in a great measure, from a weak or unnatural constitution of the nerves; and of this kind, I presume, are most of those symptoms which physicians have commonly distinguished by the names of *flatulent*, *spasmodic*, *hypochondriac*, or *hysterical*.

As the sagacious Sydenham has justly observed, that the shapes of proteus, or the colours of the chamæleon, are not more numerous and inconstant, than the variations of the hypochondriac and hysterical disease*; so those morbid symptoms which have been commonly called *nervous*, are so many, so various, and so irregular, that it would be extremely hard, either rightly to describe, or fully to enumerate them. They imitate the symptoms of almost all other diseases; and, indeed, there are few chronic distempers with which they are not more or less blended or intermixed. Hence it is, that the late celebrated Dr Mead says of the hypochondriac affection, *Non unam sedem habet, sed morbus totius corporis est*†. I shall not, therefore, undertake to give a full or exact description of these disorders, nor pretend to exhibit a compleat list of all the morbid symptoms which have been commonly reckoned of the nervous, hypochondriac, or hysterical kind; but shall content myself with mentioning the following, as being the most common and remarkable.

WIND in the stomach and intestines, heart-burning, sour belchings, squeamishness, and vomiting of a watery stuff, tough phlegm, or a black liquor like the grounds of coffee; want of appetite and indigestion, or an uncommon craving for food and quick digestion; a debility, faintness, and sense of great emptiness about the stomach, when hungry; a strong desire for rare or uncommon sorts of food, or for things that can afford no nourishment; a visible swelling or inflation of the stomach, especially after eating; sometimes a severe pain with cramps in that *viscus*; an oppression about the *præcordia*; an uneasy, though not painful sensation about the stomach, attended with low spirits, anxiety, and sometimes great

* Sydenham. oper. epist. ad D. Cole.

† Monita et præcept. med. cap. 17.

great timidity ; strong pulsations within the belly ; spasms in the bowels, and distensions of certain portions of them ; violent cholic pains ; a rumbling noise from wind passing through the intestines ; the body sometimes too lax, oftener bound ; pains in the back and belly, resembling those of the nephritic kind ; a sense of irritation and heat in the neck of the bladder and *urethra*, with a frequent desire to make water ; a great discharge of limpid urine ; at other times a frequent spitting.

SUDDEN flushings of heat over the whole body, shiverings, a sense of cold in certain parts, as if water was poured on them, at other times an unusual glow ; flying pains in the arms and limbs ; a troublesome pain in the back, and between the shoulders ; pains attended with a hot sensation, shifting often from the sides or back to the interior parts of the *abdomen* ; cramps, or convulsive motions of the muscles, or of a few of their fibres ; sudden startings of the legs and arms ; almost constant involuntary motions of the muscles of the neck and head, or arms and legs ; a general convulsion affecting, at once, the stomach, bowels, throat, legs, arms, and indeed almost the whole members of the body, in which the patient struggles as in a violent epileptic fit ; long faintings, in some cases, following one another, after short intervals.

PALPITATIONS, or trembling of the heart ; the pulse very variable, frequently natural, sometimes uncommonly slow, and at other times quick, oftener small than full, and, on certain occasions, irregular or intermitting ; a dry cough, with difficulty of breathing, or a constriction of the lungs, sometimes returning periodically ; yawning, the hiccup, frequent sighings, and a sense of suffocation, as if from a ball or lump in the throat ; fits of crying, and convulsive laughing. Although in the day-time the patients are generally pretty cool, and the pulse sometimes slower than natural, yet in the night, especially in time of sleep, hot flushings often spread over almost the whole body, the pulse becomes quicker and stronger, and a faintness, or some degree of sickness at the stomach is felt.

A giddiness, especially after rising up hastily ; pains in the head,

sometimes returning periodically; a violent pain in a small part of the head, not larger than a shilling, as if a nail was driven into it; a ringing in the ears; a dimness of sight, and appearance of a thick mist, without any visible fault in the eyes. Objects are sometimes seen double, and unusual smells are perceived; obstinate watchings, attended sometimes with an uneasiness which is not to be described, but which is lessened by getting out of bed; disturbed sleep, frightful dreams, the night-mare; sometimes a drowsiness, and too great inclination to sleep; fear, peevishness, sadness, despair, at other times high spirits; wandering thoughts, impaired memory, ridiculous fancies; strange persuasions of their labouring under diseases of which they are quite free; and imagining their complaints to be as dangerous as they find them troublesome; they are often angry with those who would convince them of their mistake.

PATIENTS, after having been long afflicted with many of these symptoms, (for all of them never happen to any one person), sometimes fall into melancholy, madness, the black jaundice, a dropsy, tympany, *phthisis pulmonalis*, palsy, apoplexy, or some other fatal distemper.

THOSE patients who are liable to the above complaints, some of which deserve the name of *nervous* much better than others, may be distinguished into three classes.

1. SUCH as, though usually in good health, are yet, on account of an uncommon delicacy of their nervous system, apt to be often affected with violent tremors, palpitations, faintings, and convulsive fits, from fear, grief, surprise, or other passions; and from whatever greatly irritates or disagreeably affects any of the more sensible parts of the body.

2. SUCH as, besides being liable to the above disorders from the same causes, are almost always, more or less, troubled with indigestion, flatulence in the stomach and bowels, a lump in the throat, the *clavus hystericus*, giddiness, flying pains in the head, and a sense
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of cold in its back part, frequent sighings, palpitations, inquietude, fits of salivation, or pale urine, &c.

3. SUCH as, from a less delicate feeling or mobility of their nervous system in general, are scarce ever affected with violent palpitations, faintings, or convulsive motions, from fear, grief, surprise, or other passions; but, on account of a disordered state of the nerves of the stomach and bowels, are seldom free from complaints of indigestion, belching, flatulence, want of appetite, or too great craving, costiveness, or looseness, flushings, giddiness, oppression or faintness about the *præcordia*, low spirits, disagreeable thoughts, watching or disturbed sleep, &c.

THE complaints of the first of the above classes may be called *simply nervous*; those of the second, in compliance with custom, may be said to be *hysteric*, and those of the third, *hypochondriac*.

THE hypochondriac and hysteric diseases are generally considered by physicians as the same; only in women, such disorders have got the name of *hysteric*, from the antient opinion of their seat being solely in the womb; while in men, they were called *hypochondriac*, upon the supposition, that in them they proceeded from some fault in those *viscera* which lie under the cartilages of the ribs.

THE learned Hoffman, dissenting from most of the later writers, affirms, that the hypochondriac and hysteric are different diseases, whether we regard their symptoms, causes, or termination*: But we cannot agree to this opinion, as their symptoms are of so similar a nature, and as the hypochondriac disease is not more unlike the hysteric, than this last is often unlike to itself. It is true, that in women, hysteric symptoms occur more frequently, and are often much more sudden and violent, than the hypochondriac in men; but this circumstance, which is only a consequence of the more delicate frame, sedentary life, and particular condition of the womb, in women, by no means shews the two diseases to be, strictly speaking,

* *System. med. tom. 3. p. 4. cap. 5. § 5. et 6.*

speaking, different. Nor does it appear more reasonable to pronounce the hysteric disorder of a different kind from the hypochondriac; because the former may have its seat frequently in the *uterus*, and the latter in the alimentary canal, than it would be to distinguish the hypochondriac complaints into as many different diseases as the causes from which they may arise; or to divide hysteric fits, as they are called, in women, into nervous, stomachic, and hysteric, because they often proceed from violent affections of the mind, or a disordered state of the stomach, as well as from a fault in the *uterus*.

BUT further, it is to be observed, that in women the symptoms commonly called hysteric, are less frequently owing to the unsound state of the womb than to faults somewhere else in the body; for virgins are often free from such complaints, while married women, and even those who bear children with easy labours, are sometimes afflicted with them. Add to this, that women who are regular, and have no ailment about the *uterus*, do not always escape the hysteric disease; while those who labour under scirrhus tumours, and other disorders of that part, are often not affected, at least, with its worst symptoms. Lastly, in those who have long and greatly suffered by this malady, the womb, after death, has frequently been observed to be sound.

UPON the whole, therefore, the symptoms of the hysteric disease in women, seem only to differ from those of the hypochondriac in men, in so far as the former sometimes proceed from the *uterus*, and are, on account of the more delicate frame of the sex, more frequent and often more violent than the symptoms of the hypochondriac affection in men.

BUT whether these two distempers be considered as the same or distinct, since the symptoms of both are so much a-kin, we shall consider them under the general character of NERVOUS; and begin with inquiring into the causes from which they most commonly proceed.

THE antient physicians, with several of the moderns, have agreed in placing the sole, or at least the chief seat of the hysteric disease
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in the womb ; but, with regard to the parts affected in the hypochondriac, the opinions have been various and contradictory.

MANY authors have ascribed this disorder in men to obstructions in the spleen, liver, and mesentery : Highmore, to a vitiated constitution of the stomach *. Willis, to an indisposition of the brain and nerves, or to a fault of the spirits. Etmuller, who confounds the hypochondriac disease, when in a higher degree, with the scurvy, has written a dissertation to prove that its seat is not in the spleen, but in the intestines, especially in that part of the *colon* which lies in the left hypochondre, in which the excrements often stagnate, and where much wind is pent up † : Sydenham ascribes the same distemper to an ataxy or confusion of the spirits ‡ : Mandeville, to a disordered chylication, and a deficiency or paucity of the spirits || : Junckerus makes the *causa proxima* of the hypochondriac affection to consist in an obstructed motion of the blood in the *vena portarum* and *viscera* connected with it **. Boerhaave derives it from an atrabiliary humour lodging in the *pancreas*, spleen, stomach, and neighbouring organs †† : Hoffman, from a perverted peristaltic motion of the stomach and intestines ‡‡. And lastly, Dr Cheyne is of opinion, that all great nervous disorders proceed from some glandular obstruction in the stomach, bowels, liver, spleen, mesentery, or other organs of the lower belly |||.

BUT altho' it is not to be doubted, that the hypochondriac and hysteric affections often proceed from a morbid state of the alimentary canal, *uterus*, or other *viscera* of the *abdomen* ; yet as there are several of their symptoms which seem independent of any disorder in those parts, and as there has often no trace of those diseases appeared after death in any of the abdominal organs, it seems highly

* Exercitationes de passione hyst. et affection. hypochondr.

† Oper. p. 1820.

‡ Epist. ad D. Cole.

|| A treatise of the hypochondriac and hysteric passions, dialogues 1, and 2.

** Junckeri Conspect. medicinæ, p. 186.

†† Aphorism. de cognoscend. morb. § 1098.

‡‡ System. med. tom. 3. part 3. c. 5.

||| English malady, part 2. chap. 7.

highly probable, that they may frequently arise from some less visible fault in the body.

WE shall therefore proceed to inquire into the most common causes of those nervous, hypochondriac, or hysteric symptoms above mentioned, treating first of such causes as render the body more liable to these disorders; secondly, of those which, meeting with the former, actually produce them. The first have been called the predisposing causes; the second the occasional causes.

C H A P.

C H A P. III.

Of the Predisposing CAUSES of NERVOUS, HYPOCHONDRIAC, and
HYSTERIC DISORDERS.

THESE may be reduced to two, *viz.*

I. A too great delicacy and sensibility of the whole nervous system.

II. AN uncommon weakness, or a depraved or unnatural feeling, in some of the organs of the body.

I. A too great delicacy and sensibility of the whole nervous system may be either natural, that is, an original defect in the constitution, or produced by such diseases, or irregularity in living, as weaken the whole body, especially the nerves. Long or repeated fevers, profuse hæmorrhages, great fatigue, excessive or long continued grief, luxurious living, and want of exercise, may increase or even bring on such a delicate state of the nervous system.

As the whole animal frame is contrived with the greatest wisdom, so we cannot but admire, in particular, how the nerves, though all are endued with the general sense of feeling, have yet in different organs certain sensations quite different from each other, and are perfectly well adapted to those things which are designed by nature to be applied to them. Thus, for example, as pure air gives no uneasiness to the nerves of the wind-pipe, and is refreshing to those of the lungs; so to a craving stomach wholesome food is highly grateful: But air collected in the stomach seldom fails to produce a disagreeable sensation, and not only solid food, but even the mildest liquids, falling by accident into the wind-pipe, bring on violent fits of coughing, which do not cease till the offending cause is removed.

moved. In like manner warm blood, which does not affect the heart or vascular system with any disagreeable sensation, occasions in the stomach faintness, severe sickness, and vomiting. The nerves of the nose, tongue, and stomach, are all endowed with sensations of different kinds ; whence some substances, very ungrateful to the palate, are often agreeable to the stomach. Several substances which hurt the eyes, give no uneasiness to the alimentary canal ; and on the other hand, antimonial wine, or an infusion of ipecacuanha in water, which neither irritate much the tongue, or other sensible parts, affect the stomach so disagreeably as to occasion violent vomiting.

BUT further, as the nerves, in many of our organs, have very different feelings ; so in different people, or even in the same person at different times, the feeling of the same nerves varies considerably, and is more or less acute or blunt, and sometimes unnatural or depraved. And hence it is, that the very same things applied to the same nerves, or organs, have very different effects, according to the constitution of the persons, or their state of health at the time.

IN some the feelings, perceptions, and passions, are naturally dull, slow, and difficult to be roused ; in others, they are very quick and easily excited, on account of a greater delicacy and sensibility of the brain and nerves.

ALL children, when compared with adults, have their nervous system very sensibly and easily moved, and are in this respect somewhat like those grown people who are most subject to the highest nervous or hysteric symptoms : And hence it is, that children are so liable to convulsive fits from the pain of teething, from worms, acrid humours in their stomach or bowels, and other causes, which, in people of a more advanced age, and less sensible nerves, would produce no such effects.

A delicate or easily irritable nervous system, must expose a person to various ailments, from causes affecting either the body or mind, too slight to make any remarkable impression upon those of firmer and less sensible nerves. Thus, any accident occasioning sudden

sudden surprise, will, in many delicate people, produce strong palpitations of the heart, and sometimes fainting with convulsions. I have known some, even men, whose nervous systems were so delicate and moveable, that a vomit, a smart purge, or the pain raised by a blister, would throw them into convulsive fits. Nay, there was lately a paralytic patient in the Royal Infirmary here, who felt a remarkable uneasiness through his whole body, when it was charged with the electrical fluid, by means of a wire held in his hand, altho' there was no shock given him, nor any sparks drawn from him. We are told of a lady, who, upon hearing the sound of a bell, or any loud noise, used to fall into fits of swooning, which were scarce to be distinguished from death*: And I have seen the pain of the toothach throw a young woman, of weak nerves, into convulsions and insensibility, which continued for several hours, and returned upon the pain becoming again more acute†.

SOME women, from a too great delicacy or sensibility of the nervous system, are, after conception, so much affected with a heat

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* Boyle's Usefulness of exp. philosophy, part 2. p. 248.

† The following case, communicated to me by Mr James Spence surgeon in Dunkeld, is a remarkable instance of the many violent and uncommon symptoms which may arise from a small cause, in persons of a very delicate nervous system.

An unmarried woman of 23 years of age, immediately after having been stung in the neck by a bee, felt a sharp pain, with a violent itching in that part, and over the whole head and face, which, together with her arms, felt stiff and swelled. In a few minutes, the pain spread to her throat and then to her stomach, occasioning a great anxiety and difficulty of respiration. At this time, a large dram of malt spirits was given her, which, tho' it was immediately vomited up again, relieved the pain for a little; but soon after it was felt violently in the lower belly, and was followed by a loose stool. She complained now of an uncommon heat in her face and head, and of a great faintness: Her pulse was small and irregular, her tongue and throat dry, her extremities cold, and the whole body affected with a tremor. After taking a draught of warm water, and having the part that was stung rubbed with warm oil of olives, she was put to bed, and found considerable relief from flannel-cloths, wrung out of a hot decoction of some emollient herbs, applied to the *abdomen* and feet. After this, a draught with some of the *elixir paregoricum* soon produced a profuse sweat, and freed her from the pain, inclination to vomit, and other symptoms. Next day her skin being hot, and her pulse full, a sweat was again procured by a draught with *sp. minder.* and *sal. vol. ammon.* and, before the evening, she was free from every complaint.

and uneasy sensation in their back, colic-pains, and other symptoms, as to be in hazard of miscarriage. In such cases, when the danger neither arises from too much blood, nor too great a laxity of the uterine vessels, but merely from an uncommon weakness and delicacy of the nerves, bleeding will do harm, and astringent and cooling medicines will prove ineffectual, whilst *laudanum* given from time to time, in proper doses, will produce the best effects: For, by lessening the too great sensibility of the nervous system, it not only quiets all the uneasy sensations, but calms the mind itself, and renders it less liable to be ruffled by slight causes.

WOMEN, in whom the nervous system is generally more moveable than in men, are more subject to nervous complaints, and have them in a higher degree. On the other hand, old people, in whom the nerves have become less sensible, are little afflicted with those disorders; nay, Dr Cheyne has observed, that an advanced age sometimes proves a cure.

LASTLY, altho' the variolous matter in the blood, by its *stimulus*, frequently produces, in children, convulsions before the eruption; yet, in grown people, whose nerves are less delicate, this symptom rarely, if ever, happens. On the other hand, people whose solids are less firm, and their nerves more delicate and easily affected, although subject to many complaints, yet are seldom attacked with ardent fevers or violent inflammatory diseases; which seems to be chiefly owing to the weak state of their blood and vessels.

To the different sensibility of the nerves in general, or, at least, of those of the heart, is owing, in a great measure, the variety of the quickness of the pulse in healthy people. A late physician of this place told me of one of his patients, whose pulse, in a healthy state, did not beat above 38 or 40 times in a minute: And I know a young woman, whose natural pulse, when sitting, is rarely under 120, yet has no complaint, and seems to enjoy good health: Near nine years ago, when I attended her in a fever, her pulse beat upwards of 180 in a minute; and she was, at that time, troubled with the greatest startings and tremors I had ever seen: Nay, so very irritable was her heart, that after the fever was much abated, and
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when in a horizontal posture, her pulse beat under 140, by only fitting up in bed for a little while, it became so quick that, with difficulty, I could count it; but, after repeated trials, found it to be nearly 220 in a minute.

Is not the quickness of the pulse, in children, chiefly owing to the greater sensibility of their hearts? and does not the pulse generally grow slower with age, because the heart becomes less sensible, and, in a very advanced age, perhaps, in some degree, callous? Lastly, is not the pulse, *ceteris paribus*, quicker in small than in large animals, chiefly because the nerves are endowed with a greater degree of sensibility in the former than in the latter*?

SINCE, as we have observed, the nerves in the different organs are endowed with various kinds of feeling, and are very differently affected by the same things, will not morbid humours in the blood be more apt to produce diseases in those parts whose nerves are most strongly affected by them, than in others which suffer less? And may not this be, partly, the reason why, in certain diseases, some parts of the body are much more commonly affected than others? And why, in some epidemics, the eyes, nose, or *fauces*, and, in others, the breast or intestines are most apt to suffer? This also may, partly, be the cause why those organs which have suffered by some former diseases, are most liable to be attacked, when the body is seized with any new disorder; for this does not seem to be owing solely to the weakness of the vessels, but also to their being more easily irritated by any acrimony in the blood, or by its increased force. Further, it may be proper to take notice here, that the different operations of various medicines are not so much owing to their powers, either of dissolving the blood, or changing it in other respects, as to the particular nature of the nerves of the different organs, disposing them to be very differently affected by the same kind of stimulating substances.

THUS cathartic medicines applied to the belly of children, in the form

* The slowness of the pulse in larger animals is, no doubt, partly owing to the ventricles of their heart, on account of their greater capacity, requiring a longer time for the performance of their several motions.

form of a plaister, do not sensibly increase the secretion from the liver, or from the salivary or lachrymal glands; but they so affect the nerves of the intestines, as to occasion a greater flux of humours from their vessels, and accelerate the peristaltic motion, and so bring on a purging: and this does not seem to be owing so much to the finer parts of those medicines, which enter the blood, and may be conveyed with it to the bowels, acting immediately on their nerves or small vessels, as to a particular sympathy between the nerves distributed to the teguments of the *abdomen* and those of the intestines; otherwise an aloetic plaister applied to the back or the head should open the body as much as when laid to the belly. Nitre, which proves often highly diuretic, does not seem to affect the secretions of the other glands remarkably. The finer parts of *cantharides* entering the blood by the application of blisters, rarely produce vomiting or purging, or disagreeably affect any part, except the urinary passages, where the nerves are so formed, as by the acrimony of the flies to be more irritated than those of the other organs. Nor can the stranguary, occasioned by *cantharides*, be owing, as some have thought, to their particles not passing freely through the vessels of the kidneys and bladder, since the vessels of the brain are much smaller than these, and since the kidneys are not near so much affected by them as the neck of the bladder. Does not mercury, when mixed with the blood, generally increase the secretion of the *saliva* much more than that of any other humour, because the small vessels of the salivary glands are more strongly affected by its peculiar *stimulus*, than those of any other secretory organ? Lastly, Does it not appear, from what has been said, that the virtue of a medicine, which is specifically to promote the secretion of the bile, *semen*, urine, or the *saliva*, must consist in its being peculiarly fitted for stimulating, and consequently increasing the vibratory motions of the small secreting vessels of the liver, kidneys, testicles, or salivary glands, more than those of the other parts? And do not such medicines alone, if any such there be, deserve, in a strict sense, the name of Emmenagogue, which not only tend, by their general stimulating or attenuating power, to promote

promote the menstrual evacuation, but also, by their particular quality, are fitted to stimulate the nerves and vessels of the womb more than those of any other *viscus*?

BUT, to return from this digression :

II. BESIDES a too great sensibility of the nervous system in general, there is often an uncommon weakness or delicacy, or an unnatural or depraved feeling in various parts of the body, which exposes certain persons to violent, and sometimes very extraordinary affections, from causes which would scarce produce any disturbance in people of a sound constitution.

THUS, several delicate women, who could easily bear the stronger smell of tobacco, have been thrown into fits by musk, ambergrease, or a pale rose, which to most people are either grateful, or, at least, not disagreeable. The smell of cheese has, almost always, occasioned a bleeding of the nose in some *. Mr Boyle tells of a nobleman, who was apt to faint away when tansy was brought near him; and there lately lived in this country a lady, who was affected with a general uneasiness, as often as there was any fellery in the room where she sat. The sight of a cat, nay, even the invisible *effluvia* from that animal, have occasioned anxiety, faintness, and sweating †. I had, several years since, a patient who was always affected with an itching and uneasiness over her whole body, when she either swallowed nutmeg, or applied it externally. There have been some who were very ready to faint when they smelled to cinnamon: and Mr Boyle mentions a lady who had such an antipathy to honey, that a little of it put into a poultice, without her knowledge, and laid to a slight wound, threw her into great disorder, which continued until that application was removed ‡. I knew a woman who, soon after conception, always contracted an aversion to snuff, and did not recover her taste for it, until some time after her delivery: And it is well known, that, in time of pregnan-

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* Kaau Boerhaave *Impet. facies*, § 409.

† Ibid.

‡ Usefulness of experimental philos. part 2. p. 260.

cy, the nerves of the stomach are so much changed, that most women are then troubled with a *nausea*, vomiting, or depraved appetite. Lastly, certain persons, in consequence of an uncommon delicacy, or unnatural sensibility of the nerves which terminate in the *bronchia*, or vesicles of the lungs, are apt to suffer an asthmatic fit from the *effluvia* of particular substances, which produce no such effect on those whose pulmonary nerves are differently disposed.

BUT there is no organ of the body, the unnatural state of whose nerves is so frequently the cause of nervous, hypochondriac, and hysterical disorders, as the alimentary canal, especially the stomach.

AN uncommon delicacy of the nerves of the stomach and intestines, which may be either in a great measure natural or brought on by diseases, improper aliment, irregular living, excessive grief, or other causes, is to be distinguished from that acute feeling, or increased sensibility, which is the consequence of an inflammation, or of an aphthous state of these parts, since in these last cases every acrid substance gives them pain; whereas, in the former, many insipid and seemingly innocent aliments produce great uneasiness in the stomach and bowels, while volatile spirits, strong wine, brandy, and spices, are not only inoffensive, but often necessary for allaying those disorders which are produced in the first passages, by such causes as would scarce give any disturbance in a sound state.

FURTHER, This morbid or delicate state of the stomach and bowels does not consist solely in their weakness, but chiefly in the uncommon disposition of their nerves, which have a feeling very different from what is natural. As a proof of this, we observe, that in such a state of the alimentary canal, the appetite is often not only good, but beef and mutton, even when salted and dried, will be more easily digested, and give less disturbance than many vegetables, which in healthy persons sit much lighter on the stomach*.

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* It is a mistake to think, as some have done, that vegetable food in general is worse to digest than animal. The contrary seems to be demonstrated by Walæus's experiments on dogs;

It is surprising, how much the condition of the stomach and intestines, and the disposition of their nerves, will vary, even in the same persons at different times.

Thus cabbage, onions, leeks, and other vegetables, will lie long on the stomach, and occasion flatulence and loose stools in many, who formerly found no such inconvenience from them; and the same thing is true of honey and other aliments: nay, Mr Boyle tells us of a person, who was more violently vomited by coffee than *crocus metallorum*, or other strong emetics; and was made sick even by the smell of this liquor, as he passed by a coffee-house, altho' formerly he used to drink it without feeling any disagreeable effects *. In some people the state of the nerves of the stomach is so very uncommon, that *laudanum*, instead of relieving, will excite vomiting, and occasion violent cramps in that organ: nay, there have been persons with whom pills of *opium* always disagreed when newly made; but occasioned no disturbance after being kept some weeks.

THAT many of those complaints which have been commonly called *nervous* proceed, in a great measure, from a particular unnatural or depraved sensibility of the nerves of the alimentary canal, appears evidently from this, that, altho', in many cases, the stomach and intestines are much diseased, yet the patients are not affected with any remarkable nervous or hypochondriac symptoms, while others are greatly troubled with these complaints who have a good appetite, a quick digestion, and no tough phlegm, or other noxious humour in their stomach. Add to this, although children,

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dogs; from which it appears, that bread and herbs are much sooner digested than butcher-meat, even by those animals which are naturally carnivorous; the former remaining in the stomach only four or five hours, and the latter seven or eight. *Vid. Epist. de mot. chyl. et sang. ad Thom. Bartholin.* Agreeably to this, people whose stomach and intestines are quite sound, find themselves lighter, and much sooner hungry, after a dinner of white bread, herbs, roots, or ripe fruit, than one of beef, mutton, or pork. It is not owing, therefore, to their being more difficult to digest, or their remaining longer in the stomach, that many vegetable aliments give such disturbance to some delicate people, but to their affecting disagreeably the nerves of the alimentary canal. For the same reason it is, that roasted meat agrees better with them than broth or boiled meat, and old cheese than new pressed curds.

* Usefulness of experimental philosoph. part 2. p. 260.

dren, on account of the great sensibility of their nerves, are liable to convulsive disorders, and other nervous complaints; yet they are rarely affected with the hypochondriac disease, because the nerves of their stomach and intestines have not that unnatural or depraved feeling which is common in this malady; and which, when it is on certain occasions much increased by some acrid matter in the blood falling on them, becomes not only the predisposing cause, but constitutes the hypochondriac disease itself, and gives rise to most of its symptoms.

IN a weakly and delicate, or an unnatural state of the stomach and bowels, improper aliments, excess in eating or drinking, wind, sharp humours, and strong passions, such as grief, anger, and the like, will occasion much more violent symptoms than in persons whose alimentary canal is firm and sound. Thus, a draught of cold water will instantly affect some very delicate women with a violent pain and cramp in their stomach; and the sight of one vomiting, or of certain disagreeable aliments, or medicines, will produce a *nausea*, and even vomiting, in persons whose stomachs are easily moved. Nay, in some cases, so very delicate is the state of the stomach, that turning the body hastily in bed, or raising one's self, will immediately occasion a faintness, giddiness, a general weakness, and sometimes an inclination to vomit. This last symptom has been remarked by Sydenham in hysteric women; and I have had several patients in continued fevers, who, together with an uncommon debility and faintness, were, upon the smallest motion in bed, seized with a *nausea* and retching to vomit.

FURTHER, A delicate state of the first passages, or an unnatural sensibility of their nerves, not only disposes people to many complaints in those parts, but the whole nervous system is thereby rendered more moveable, and liable to be affected by the slightest causes. Thus, I have known some women of a delicate frame, in whom, from an obstruction or irregularity of the *menstrua*, the nerves of the stomach had acquired such an uncommon sensibility, that, after eating freely of any solid meat, they were not only seized with a pain and sickness at the stomach, and a sense of stiffness

ness and rigidity in the trunk of the body, but sometimes also with faintings, attended with a quick trembling pulse, and slight convulsions of the muscles of the legs and arms. A woman of a delicate constitution, who was attacked with a quotidian intermittent, seven weeks after child-bearing, as often as she swallowed some *magnesia alba*, felt immediately a kind of quivering motion propagated thro' her whole body. The same person, as often as she took a draught of lime-water, observed the palms of her hands, which before were soft and moist, become at once dry and hard. It was remarkable, that neither crabs eyes, nor chalk, occasioned any such uneasy feeling as the *magnesia* did.

WHEN my stomach and bowels have been out of order, and affected with an uneasy sensation from wind, I have not only been sensible of a general debility and flatness of spirits, but the unexpected opening of a door, or any such trifling unforeseen accident, has instantly occasioned an odd sensation about my heart, extending itself from thence to my head and arms, and, in a lesser degree, to the inferior parts of my body. At other times, when my stomach is in a firmer state, I have no such feelings, or, at least, in a very small degree, from causes which might be thought more apt to produce them.

FROM what has been said, we may see, that faintings, tremors, palpitations of the heart, convulsive motions, and great fearfulness, may be often owing more to the infirm state of the first passages, than to any fault either in the brain or heart. But it would be unnecessary to insist farther on this head, as the powers which the alimentary canal, when its nerves are disagreeably affected, must have in producing disorders in the most distant parts of the body, cannot be doubted of by those who attend to that wonderful and widely extended sympathy, which obtains between it and almost the whole system *. What has been said may be sufficient to shew, how much a delicate or unnatural state of the nerves of the alimentary canal must dispose people to nervous, hypochondriac, and hysteric complaints. But further, when, through the fault of the stomach

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* See above, chap. I. No. II.

and intestines, the digestion is imperfectly carried on, the ill prepared chyle may lay a foundation in the blood for exciting a variety of nervous symptoms, as will afterwards more fully appear.

SINCE the stronger or weaker effects of emetics and cathartics must depend entirely on the different constitution of the nerves of the *primæ viæ*, and the quantity of *mucus* defending them, it is easy to see that the doses of those medicines can neither be certainly determined by the ages nor sizes of the patients, nor by the quantity of blood in their vessels.

It is owing alone to the different sensibility which the nerves of the alimentary canal in different persons have of various *stimuli*, that the several vomiting and purging medicines have such different effects: That the strongest emetics scarce move some people, while, in others, the mildest are apt to have too great an operation: That a few grains of rhubarb shall purge and gripe one patient severely, and a drachm of the same medicine have no sensible effect on another: That a drachm and a half of soluble tartar shall prove a stronger purgative to some, than four ounces of sacred tincture: That children are often harder to purge than some adults*: That worms, tough phlegm, and other noxious humours, lodging in the stomach and bowels, produce very different effects in different persons; and that the bark, which generally makes the body costive, occasion gripes and purging in some. And is it not to be ascribed chiefly, if not solely, to the different constitution of the nerves in different animals, that what is highly noxious to some, proves wholesome food to others? Thus, the *cicuta aquatica*, which is eaten by goats without any harm†, is a deadly poison to men and other animals.

WHEREIN consist the various kinds and degrees of sensibility, which the nerves of the alimentary canal and other organs possess,
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* It is here to be observed, that in children frequently, and also sometimes in adults, vomiting and purging medicines have much less effect than might be expected, considering the delicacy of their nerves, on account of the stomach and intestines being lined either with a great deal of natural *mucus*, or morbid slime.

† Swencke Dissertat. de cicut. aquat. Gesneri.

we no more know, than we do their peculiar structure, or how they come to be endued with sensation at all: but that the particular sensibility of the nerves of the gullet, stomach, and intestines, is often greatly changed by diseases, even when the nervous system in general is not much altered, we know from experience*. Nor is there, perhaps, to be found a stronger instance of this than in the *hydrophobia* consequent on the bite of a mad dog; where the purest water excites such convulsive motions of the gullet, stomach, diaphragm, and abdominal muscles, that, after a few attempts to swallow it, the sight of any fluid, and especially if it touches the patient's lips, will instantly affect him with horror, and throw him into violent convulsions and vomiting. In some cases (altho' these more rarely happen) the nerves also of the intestines become so far depraved in their feeling, that liquors can no more be admitted by injection into the great guts than into the stomach by deglutition. Nay, it should seem that, sometimes, not only the nerves of the alimentary canal are strangely altered in this disease, but also those of the face, and perhaps of the whole surface of the body, since we are told of hydrophobic patients, who could not even bear a blast of cool air†.

How this change is produced in the nerves of the first passages, or other parts in the *hydrophobia*, or in what it consists, is perhaps one of those difficulties which physicians may despair of being ever able to explain. One thing, however, is certain, that, in men as well as dogs, who have died of that disease, the gullet and stomach have been often found free from any visible inflammation; whence the disease must have had its seat either in the nerves themselves, or in vessels smaller than those which carry red blood. But
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* Since it is probable, that the nerves are partly nourished by the fluids distributed to that production of the *pia mater* which surrounds their medullary substance, it is easy to see that the nerves of a particular organ may have their sensibility increased, diminished, or otherwise changed, by fluids that are improper, or of an acrid nature, being sent to them; when, in the mean time, the brain and nervous system in general may be sound, and suffer in no other way but by sympathy with that organ whose nerves are morbidly affected.

† Philosophical Transactions abridged, vol. 5. p. 366. and Act. acad. Moguntin. tom. 1. p. 341.

whatever may be the change made by this distemper on the nerves of the alimentary canal, or in what manner soever the canine poison produces this change, we know that if, from any cause, the nerves of the *fauces*, gullet, and stomach should acquire a sensibility, something similar to that which the nerves of the *larynx* and *trachea* are naturally endued with, the most violent convulsive motions of those parts, and retchings to vomit would ensue, upon attempting to swallow even the mildest liquors. In this, however, the sensibility of the *fauces* and gullet, in the *hydrophobia*, differs from that of the *larynx* and *trachea* in a natural state, that these last parts suffer still more from solids than liquors of a mild nature; whereas the former are disagreeably affected by liquids alone.

BUT to return. As a too great sensibility of the nervous system in general, or an unnatural delicacy of the stomach and intestines, or other organs, in particular, do not commonly of themselves produce those various symptoms which go by the name of nervous, hypochondriac, and hysteric, I come next to inquire into those several occasional causes, which, meeting with the predisposing ones above mentioned, may bring on this numerous train of diseases.

C H A P. IV.

Of the general occasional Causes of NERVOUS, HYPOCHONDRIAC, and HYSTERIC Disorders.

THESE are either to be found in the blood, or they have their seat in some particular organ of the body. The former I shall call general, the latter particular occasional causes.

THE general occasional causes may be reduced to three, *viz.*

- I. SOME morbid matter bred in the blood.
- II. THE diminution or retention of some accustomed evacuation.
- III. THE want of a sufficient quantity of blood, or of blood of a proper density.

I. SOMETHING bred in the blood, and not carried off by any of the excretories, disagreeably affecting the nerves, as often as it comes into contact with them; or forming obstructions in the small vessels, and producing different symptoms, according to the parts it attacks.

THAT many of the symptoms commonly called nervous, hypochondriac, or hysteric, are frequently owing to some noxious matter in the blood, affecting, at different times, different parts of the body, I have been fully convinced by many cases which have occurred in my practice; but shall only mention two, which seem to prove this point sufficiently.

1. A boy, of ten years of age, of a very sensible nervous system, who, in December 1747, had been seized with a palpitation of his heart, fell from his horse about the beginning of January. From this time the palpitation left him; but, in a few days after, he was attacked with a violent headach, returning sometimes
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once a day, at other times only every 3d or 4th day. During the fit, his pulse became smaller and quicker, and often intermitted; his feet were cold, but, by the violence of the pain, a plentiful sweat broke out and relieved him. As these headachs continued to increase, the patient lost his stomach and flesh, and looked pale. By the use chiefly of an electuary of the bark and valerian, in less than three weeks the pain in the head abated greatly; but his appetite grew worse, and he often complained of a *nausea*. These symptoms, however, were all removed, in four or five days, by some warm stomachic and cordial medicines; but were succeeded by an intolerable pain across the middle of his belly, which, in the space of eight days, returned five or six times, and not only affected his pulse, as the headach had done, but, sometimes, occasioned a difficulty and pain in making water. This pain no sooner left his belly, than the headach returned with greater violence than ever, so that the boy used to faint in some of the worst paroxysms. It had no certain periods, coming sometimes twice a-day, sometimes only once in two days, and was attended with a sense of suffocation from wind, and a lump in his throat. He was easiest in the night when he slept or lay quiet; but any considerable motion of his body always raised his headach. Before the fits, he was observed to be uncommonly lively, and disposed to laugh. On the 21st of February, at two in the afternoon, he was seized with fits of involuntary laughter, between which he complained of a strange smell, and of pins pricking his nose; he talked incoherently, stared in an odd manner, and his complexion changed to a livid colour; immediately after he was seized with convulsions, and then fell into a fainting fit, which lasted near half an hour. When his pulse, breathing, and senses returned, he complained of a great coldness, and pain in the back part of his head, and vomited his dinner, with some tough phlegm. At this time his appetite was good, and afterwards it became greater than it used to be in perfect health.

ON the 9th of March, some purulent matter was discharged from his right nostril, and much about the same time, a small quantity
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more came from the right ear; after which he had scarce any violent fits of the headach, but a continued, tho' less severe, pain in the back-part of the head; which being greatly increased by motion, he lay constantly a-bed, and mostly on his back. Altho' he had a considerable thirst, and drank plentifully, yet, during the whole month of March, he did not make above six ounces of water in twenty-four hours, and never sweated.

ABOUT the beginning of April, the complaints of his head were so much abated, that he could bear sitting up in a chair; he began to make water more plentifully, and, when any thing ruffled him, voided great quantities of quite limpid urine. During the month of May he continued to grow better; and, before the end of June, he had perfectly recovered.

IN February 1749, he began to complain of a constant headach, which, tho' more painful at one time than another, yet was never so violent as it had been the year before, nor did it affect his pulse or stomach: But now he frequently saw objects double. In the beginning of March, some purulent matter came from one of his nostrils, and soon after the headach abated; but he lost his appetite, and was attacked with a pain in the left side of his belly, between the short ribs and *os ilium*, confined to a space little larger than the breadth of a shilling. This pain was often so severe, as to make him ready to faint: sometimes it shifted, and then he was seized with fatiguing fits of involuntary laughter. His head was always easy when the pain in his belly was worst. In the summer he recovered his health as in the year before; and next winter complained little or nothing of his head, but, for some months, had a weakness and painful feeling in his left eye, when exposed to the least light. As there was no inflammation in this eye, the pain seemed to be owing to too great a sensibility of the *retina*.

2. AN unmarried woman, aged between 25 and 30, had an irregular ague in August and September 1757, of which no symptoms remained in October, except a sweating every other day, if she lay long in bed. This she prevented by getting up before breakfast; but, in eight or ten days after, she was seized with a tightness in her
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breast, which occasioned a cough, but without expectoration. This oppression at her breast with the cough increasing, altho' the pulse was good, I thought it proper to make her lose eight ounces of blood; but neither this evacuation, nor a blister afterwards applied to her back, gave her any relief. She used a mixture with the *acetum scil-liticum*, was vomited, purged with sacred tincture, took camphire, castor, *asa fetida*, and *laudanum*, with very little benefit: At last, about the beginning of November, a musk julep taken for a fortnight almost quite freed her from her disorder.

AFTER having continued during the winter in pretty good health, she began in April to complain of pains in her legs and knees, but mostly in her body. Altho' her pulse was not altered, yet twelve ounces of blood were taken away, which had a thin fizy skin of a blueish colour. Some days after, the pain in her sides, stomach, *sternum*, and back increased, she was much troubled with wind in the first passages, and made very little water. The sense of suffocation and dry cough, which she had in October, returned; and she was seized, especially in the evenings, with such violent catchings, or convulsive motions of her legs, thighs, and almost her whole body, as not only to shake the bed, but the room in which she lay. At this time, she was vomited, blistered on the back, and took draughts of *sp. minderer.* with *sal. vol. ammon.* but without any advantage. By the use, however, of boluses of camphire and musk, with small doses of *laudanum* at bed-time, she got pretty free from the catchings; and the tightness and dry cough were also lessened; but the pains in her sides, bowels, and legs continued as bad as ever. On the 7th of May she complained of a pain and swelling in one of her arm-pits, which daily increased, and her pulse, which had generally beat only between 60 and 70 times in a minute, now exceeded 100. She lost ten ounces of blood which was very fizy; emolient fomentations and suppurating poultices were applied to the arm-pit; notwithstanding which, the pain increased to such a degree, that she was obliged to take every night a large dose of *laudanum* to procure rest. From the time this swelling and pain began under her arm, the sense of suffocation, the cough, the other pains
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and catchings abated, and left her entirely about the 20th of May, after the tumor had broke and discharged some bloody matter. During both illnesses, she continued perfectly regular.

FROM these two cases it appears, that various symptoms of the nervous kind may be owing to some morbid matter in the blood, occasioning different complaints, according to the parts upon which it falls, even when there is no reason to suspect any obstruction in the *viscera* of the *abdomen*, or fault in the *uterus*. In the first case, it is not easy to say what gave rise to the disease; but, in the second, an agueish disorder imprudently checked, leaving a taint in the blood, produced a sense of suffocation, the dry cough, pains in various parts of the body, and spasmodic contractions of the muscles; which complaints were never entirely cured, till some noxious matter was discharged by the suppuration of a gland in the arm-pit. Nor can it appear strange, that so small an evacuation should purify the blood, and relieve the patient, when, in the plague itself, a proper suppuration of one of the glands of the neck, arm-pit, or groin, will prove a perfect crisis.

As a further proof, that complaints of the nerves or hysteric kind often proceed from some morbid humour in the blood, I have frequently seen them relieved by an itching between the toes, red pustules appearing on the breast and belly, or some other cutaneous eruption.

THAT taint or morbid matter in the blood which occasions many symptoms of the nervous kind may proceed from very different causes; such as improper food, a scorbutic * or scrophulous habit, fevers which have had imperfect crises, or other diseases not fully cured, especially the cutaneous disorders; when the morbid matter, instead of being thrown off by the skin, is re-assumed into the blood, and deposited on some of the internal parts. But by far the most fre-

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* By scorbutic is not here meant, that fault in the blood which produces the true scurvy, to which people who live at sea and in marshy places are so subject, but that humour which has been commonly, though improperly, called scorbutic, and which, when it is carried to the skin, instead of livid blotches, produces dry, scurfy eruptions, scabs, tetters, &c. and, when in a high degree, the *lepra Græcorum*.

quent taint in the blood affecting the nerves, is an arthritic matter falling at different times on different parts of the body.

ARÆTEUS has long ago taken notice, that in some the gout wanders thro' the whole body* ; the truth of which observation has been confirmed by later writers †, and would have been more carefully attended to by physicians, if those symptoms which arose only from an imperfect gout had not been, for the most part, either flurred over, under the specious name of *nervous*, without any particular inquiry into their real cause, or considered merely as the effects of the hypochondriac or hysteric disease, or of the scurvy ; especially in such as, having never had a regular fit of the gout, were not suspected of any arthritic humour.

WERE it necessary, many cases might be produced to shew that nervous, hypochondriac, and hysteric complaints are often owing to an imperfect gout wandering thro' the body ; but I shall only mention the two following.

1. A gentleman aged 58, temperate, and subject to no distemper, except a rheumatism, of which, for some years, he had frequent returns in his loins, in August 1752, after a severe fit of this kind had suddenly left him, was seized with a great depression of spirits, often attended with a sickness at the stomach, and a particular sensation about the epigastric region, which he could not well describe. In less than two months, by proper medicines and exercise, he got free from those complaints ; but had not long enjoyed good health, when he began to feel frequently a slight palpitation of his heart, which was attended with an intermission of his pulse. This was succeeded

* De causis et signis morborum, lib. 9. cap. 12.

† Enimvero usu medico vel parum exercitatos, hoc latere nequit ; arthritide (præcipue frigida, inertis, languida ; maxime vero omnium ea suppressa, retusaque) ægrotantes, interdum humeri, pectoris, dorso, lumborum, aliarumque in ambitu corporis partium dolore vago tanquam rheumatico ; sæpe etiam capitis affectibus, more prorsus hystérico ; alias, aliis in corpore malis quasi scorbuticis urgeri ; sæpissime vero valetudine dubia, et in tempus diuturnum incerta, et neutra esse. Qui quidem eorum status ac conditiones, sensu remissiori et leniori gradu morbosæ natales suos arthritico miasmati, cæco, in corpus subrepenti, et eo loci clam agenti, se debere, ultro videntur agnoscere : quinetiam aliquando, multos post annos, dubium hunc in modum actos ; tandem apparente paroxysmo arthritidis idoneo, de istorum origine et natura malorum arthritica omnis sublata dubitatio est." Musgrave de arthritide anomala, cap. 19. p. 316.

succeeded by the *lumbago*, during which he found his appetite and spirits better than at other times, and indeed as good as in his best health. Afterwards, he had frequent returns of the disorder about his stomach, with low spirits, and a *nausea*, especially in the morning; and complained sometimes of a difficulty of breathing, but without any cough or spitting. This person, who never had had the gout, nor suspected it, being told, that all his complaints were owing to an arthritic matter wandering thro' his body, seemed surprised at first, but was soon after convinced, by a slight pain and inflammation which seized one of his great toes; and, during the few days it lasted, relieved him from his lowness of spirits, and complaints of his stomach. He was for several years, both before and after this fit of the gout, affected at times with a small running from the *urethra*, and a pain in the left groin, which sometimes attacked the testicle of that side. These symptoms I considered, as well as the others, to be purely arthritic, since he had never in his life had any venereal infection.

TEA, coffee, and all flatulent aliments, increased this patient's complaints. Flesh-meats, old cheese, wine, porter, and bitters with the bark, steel, and exercise, especially riding, did him most service.

2. A gentleman, aged 40, generally healthy, who, from June 1752, had been troubled with pains in his heels, and sometimes in the middle of his left foot, in the end of May 1755, about seven in the morning, awaked with an unusual sensation in his breast, and a faintness, but without any sickness at his stomach, or swimming in his head: his pulse was surprisingly irregular and intermitting. Twelve ounces of blood were taken from him, which had a natural appearance; he swallowed some warm wine and water, *sp. corn. cerv. tinct. castor.* and a solution of *asa fetida*, but without any remarkable effect.

UPON getting up, and walking thro' the room, he found himself quite free from a pain, which, for some months he had felt in the middle of his left foot. About ten, he began to make pale urine, and in five hours, voided five English pints of it, altho' what he had drunk, during this time, did not amount to half that quantity. About
noon,

noon, partly to abate this immoderate discharge, and partly to lessen the too great irritability of the heart, by bracing the *viscera* of the lower belly, he girded himself very tight with a broad belt, and, in three or four minutes after, the languor, and that unusual sensation within his breast ceased at once, and his pulse became regular and natural. Next day he began to be troubled with wind in his stomach and bowels, which did not occasion any sharp pain, but a disagreeable sensation and great lowness of spirits.

AFTER these symptoms had continued by fits for four or five days, he rode out some miles for exercise, and returned home entirely free from his complaints; only, by being exposed to a cold east wind, he caught a swelling, and a small degree of inflammation in one of his tonsils. Having supped as usual, he went to bed, and, after a short sleep, waked quite free from the inflammation in the throat, but with a great faintness, attended with a very quick and small pulse. A glass or two of claret, and a bit of bread, removed this faintness for the time; and, upon its return, it was cured by the same remedy. For some weeks after, he was much troubled with flatulencies in his stomach and bowels, with low spirits sometimes, tho' in a much less degree than before, and did not entirely recover his health and strength in several months. The pain in his heels, which he had felt but little of during most of this time, returned and continued pretty constant till the end of August 1757, when he had a slight fit of the gout, with a swelling and inflammation in his right heel. Since that period, as well as before it, he has been often troubled with a giddiness, and flying pains in his head, arms, and hands, frequent pains in his heels, and wind in the *primæ viæ*.

THESE cases need no comment. The symptoms with which the patients were affected must have proceeded from an irregular gout, the matter of which, instead of going to the extremities, wandered thro' the body. The stomach-complaints could not be owing to any tough phlegm, or other crudities; for the last person had never, in his life, thrown up by a vomit any thing of that kind; and the other, who took several vomits during his illness, never appeared to have much of a foul stomach; nay, tho' he was often oppressed with a
severe

fevere sickness, and a *nausea* in the night and morning, yet he grew easy before dinner, and then eat with as good an appetite and digestion as in his best health.

FROM what has been said, it may appear, that some morbid matter in the blood, either arthritic or of another kind, may be often the cause of nervous complaints. When this matter is carried smoothly along with the blood, without forming obstructions in any of the vessels, or irritating the nerves, it gives little trouble. When it remains fixed in the extremities, or the muscular parts of the trunk of the body, it will only occasion aching pains of the goutish or rheumatic kind: but when it is deposited on such of the *viscera* as are very sensible or by sympathy are apt strongly to affect almost the whole body, it may produce most of those symptoms which have been commonly called nervous, hypochondriac, or hysteric *. This matter may in general act either by its viscosity in obstructing the smaller vessels, and thereby stretching too much their sensible fibres and nervous filaments, or by its acrimony in disagreeably affecting the extremities of those nerves which it touches †.

IT is to be observed however, that the kind and violence of the symptoms occasioned by this morbid matter, will not only be different according to the parts which it affects, but in proportion to the greater or lesser natural delicacy or sensibility of the patient's nerves.

HENCE it seems to be, that men of otherways hale and strong constitutions, and some robust women, are liable to a regular gout, and but little to nervous complaints. Their firmer fibres and less delicate nerves do not predispose them to the latter, and the strength of

* See above, p. 530. &c.

† It is probable, that the morbid matter in the blood, producing nervous complaints, generally proves hurtful by its acrimony, and but rarely by its viscosity: at least we know, that in the small pox, measles, and continued fevers, an acrimony in the blood, by stimulating the brain and nerves, frequently produces a *delirium*, tremors, twitchings, convulsions, and other nervous symptoms: and the *horror febrilis*, or shivering upon the attack of a fever, is rather owing to a spasmodic contraction of the small vessels, than to an obstruction of them from viscid blood.

of their digestive organs, and vascular system, enables them to throw off the arthritic matter on the extremities, by which means the body is cleared of it.

MEN of a middle constitution, between the delicate and strong, are, from this morbid matter, affected with pains of the cold rheumatic kind, and various nervous symptoms in a lesser degree; and sometimes also with a fit of the true gout. But in them this distemper is not commonly so completely formed, as to clear the habit of the arthritic matter, at least for any considerable time; for soon after the imperfect fit of the gout, their old complaints begin to return.

ON the other hand, women of a more delicate habit, and men of weak fibres and very sensible nerves, have more rarely any disorder like the true gout; either because in such constitutions the arthritic matter is imperfectly formed; or, what is more probable, because the vital organs are unable to throw it off upon the joints and extremities. Hence this morbid cause in the blood, instead of being deposited on the *aponeuroses*, tendons, ligaments, and membranes of the feet, hands, or other joints, falls upon different parts of the body, and produces symptoms almost as different as are the parts which it attacks. Such, for instance, are the flying pains, spasmodic contractions, and sudden sensations of heat and cold in the muscles and exterior parts of the body. A want of appetite, or too great craving and faintness, a *nausea* or vomiting, flatulent swellings, *borborygmi*, watching, low spirits, cramps, convulsions, and violent pains in the stomach and bowels. An increased secretion of *saliva*, from an irritation of the vessels of the salivary glands. The *globus hystericus* in the gullet. A spasmodic *asthma* in the lungs. Palpitations and irregular motions in the heart. An excessive flow of pale urine, or sometimes nephritic pains in the kidneys. A *hemicrania*, the *clavis hystericus*, or shooting pains in the head. Besides these, I have seen many other symptoms, occasioned by an imperfect or an irregular gout, such as a *delirium* and *mania*, an inflammation in one of the tonsils, a troublesome *dysuria*; a violent itching between the toes; a severe pain about the *cartilago ensiformis*, returning
twice

twice or thrice a-day, especially upon any strong affection of the mind, or effort of the body, and sometimes attended with a painful sensation in the middle of each arm; a sense of a burning heat over the whole surface of the body, except the legs, while, in the mean time, the skin was scarce sensibly hotter than in a state of health, and the pulse was under 80 in a minute. In one patient I met with a slight, but frequently returning *gonorrhœa*, from a gouty humour falling on the nerves or vessels of the *urethra*; and in another an uneasy itching of the *scrotum*. I have seen three cases of a sharp pain in the testicles from the same cause. In one of these there was a considerable swelling along with the pain, both which went off upon the gout coming into both the feet.

ALL this is confirmed by observing, that persons who have been but little troubled before with those symptoms commonly called nervous, upon the rheumatic or rather gouty pains leaving their feet, hands, or loins, have been seized with an irregular intermitting pulse, giddiness, faintness, difficulty in breathing, *nausea*, and vomiting, flatulence in the stomach and bowels, depression of spirits, and other symptoms of the like kind *.

SUCH complaints, if the patient has never had the gout, are generally called nervous; but if he has been subject to it, are readily enough ascribed to the arthritic matter leaving the extremities, and fixing upon the head or *viscera* of the *thorax* or belly.

THIS difference, however, may be observed, that the symptoms arising from the retrocession of the true gout, are generally more violent than those which are occasioned by a rheumatic or imperfect arthritic humour wandering thro' the body.

UPON the whole, it may appear, that one very frequent occasional cause of many nervous, hypochondriac, and hysteric symptoms,

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* As the arthritic matter affecting the nerves of the stomach, not only occasions the symptoms now mentioned, but sometimes extraordinary languors, an universal debility, anxiety, and faintings; it is noways improbable, that the sudden death of several, subject to a wandering gout, may have been sometimes owing to its affecting the nerves of the stomach at once, and in so strong a manner, as not only to occasion fainting, but a total suspension of the motion of the heart: and this will appear still more probable by observing, that such persons

is some acrid matter in the blood, commonly no other than the arthritic humour, the cause likewise of the chronic rheumatism and true gout *.

It may be proper to observe, that, altho' a gouty humour in the blood may be much oftener the cause of nervous symptoms in men than in women; yet in the latter many complaints of this kind do certainly flow from that source. Of this I could relate many instances which have occurred in my practice; but, that I may not be tedious, I shall only mention one.

A lady aged 60, of a delicate constitution, and who had been often liable to complaints in her stomach, upon her becoming free from slight rheumatic pains, which she used to feel in her arms, began to be affected with an aversion to food, a severe sickness, and sometimes a vomiting; an acute, or burning pain in her stomach, sometimes

sons have often, immediately before their death, complained of a sharp pain or sickness or other unusual sensation in their stomach. In such cases, the cause of death will be, in vain, sought for in the heart, lungs, brain, or, indeed, in any other part of the body; for the arthritic matter affecting the stomach is too subtle to be seen, although active enough to destroy.

* It may be objected, that nervous and hysteric complaints cannot be owing to any noxious matter in the blood or finer fluids, since violent pains and other symptoms of this kind are observed to shift so suddenly from one place to another, that we can scarcely conceive this to be owing to the translocation of any morbid matter. But altho' here, as well as on many other occasions, we are obliged to own our ignorance, yet we have no more reason to deny that nervous, spasmodic, or hysteric disorders are owing to some acrid humour irritating the nerves of the parts affected, or of some other parts with which they have a remarkable sympathy, than that the gout or rheumatism proceed from such a cause, because they often move suddenly from one place to another, especially upon the imprudent application of topical remedies. When the gout leaves the head or stomach, and immediately seizes the feet; is the arthritic matter, which affected the vessels of the former parts, instantly carried to the latter? Or, is it not more reasonable to suppose, that the gouty matter, which abounds in the blood or finer fluids, as soon as it falls particularly on the feet, by exciting a great pain there, lessens or destroys the disorder in the stomach or head; and, perhaps, by removing some spasmodic contraction in their very small vessels, allows the gouty matter that was fixed in them to pass through, and mix itself with the general mass of fluids? It is further to be observed, that many symptoms of the nervous or hysteric kind seem to be owing, not to any acrid matter immediately irritating the parts which suffer, but only affecting the stomach and intestines; whence, by means of their remarkable sympathy with most other parts of the body, a variety of symptoms is occasioned, which either increase or abate, or shift from place to place, according as the nerves of the first passages are variously affected.

sometimes shifting from it to her bowels ; flatulence, belching, palpitations, and, on some occasions, a sense of faintness at the stomach, or a difficulty of breathing : After being affected with these various symptoms, which succeeded one another, without any regularity, for three or four weeks, or longer, they generally abated, and sometimes went quite off, upon sharp pains coming into the thighs, legs, and feet, which last not only felt hot, but were often swelled. I shall only add, that, as in those women who were quite regular as to the monthly evacuation, or long past that time of life when it naturally ceases, I have found hysteric complaints to be owing very often to a rheumatic or gouty humour affecting them differently at different times ; so the most attentive observation has convinced me, that by far the most frequent cause of the hypochondriac disease in men is no other than a humour of the same kind affecting chiefly the nerves of the stomach and bowels, which, from an original weakness, had been more exposed to its attacks than the other parts of the body. This humour, in those of a melancholic temperament, besides other symptoms, generally occasions watching, timidity, a great depression of spirits, and sometimes very uneasy distracting thoughts. In others of a different constitution, the same cause produces a variety of complaints in the stomach and bowels, and other parts of the body, with much less watching, and without any great degree of low spirits.

THE arthritic matter may be bred, either in consequence of some hereditary defect in the constitution, or from high living ; whereby the stomach and bowels are so weakened, or loaded with rich, heavy, or hot aliments, as to convey very improper chyle into the blood.

PHYSICIANS have widely differed about the nature of that humour which is the cause of the gout ; some making it tartareous or acid, others urinous or alkaline. But, sensible how vain all such disquisitions are, I shall not attempt to define the nature of that noxious matter in the blood, so often the cause of nervous, hypochondriac, and hysteric disorders, further than as I have already endeavoured to shew, that it is most commonly of the arthritic kind:

And I shall now add, that it may be sometimes a scorbutic or scrophulous taint, or some other fault in consequence of other diseases imperfectly cured. Indeed, there is no reason to believe, that whatever is hurtful to the human body, must be either acid or alkaline, or of some other known species of acrimony. What is the acrimony of *ipecacuanha*, antimonial wine, of *semen hyoscyami*, *opium*, *rhus*, *myrtifolia Monspeliaca*, and of the roots of the *cicuta aquatica*? Most of these substances shew no remarkable sharpness, or pungency to the taste; and yet, when received into the stomach, they quickly occasion either sickness and vomiting, raving, or insensibility, epileptic fits, or even death. What peculiar acrimony have the *effluvia* of musk, ambergrease, or a pale rose, which throw some delicate women into hysteric fits? In like manner, with regard to that morbid matter in the blood, the cause of so many nervous complaints, and even of the gout, all we know is, that it is apt to stick in the smaller vessels; that it disagreeably affects the nerves as often as it falls upon them, and thereby occasions various symptoms, more or less violent, according to the greater or lesser sensibility of the parts affected, and the constitution of the patient: But in what manner, or by means of what particular kind of acrimony, it produces these effects, we are yet entirely ignorant, and indeed likely to continue so.

II. A second occasional cause exciting nervous disorders, may be the retention of some accustomed evacuation, such as the *menfes* or hæmorrhoids.

THE *nausea*, vomiting, depraved appetite, faintings, and other complaints to which many women are liable for some months after conception, shew that a change of the circulation in the womb, an obstruction and distension of its vessels, or whatever irritates the uterine nerves, may produce many of those symptoms commonly called nervous or hysteric. The same remark may be made upon the various disorders which happen upon the suppression, diminution, or irregularity of the *menfes*, and at that time of life when this evacuation ceases. 'Tis true, these complaints are much less remarkable

able in some than in others: Thus, while many are only affected with a *nausea*, want of appetite, flatulence in the stomach and bowels, a cough, difficulty in breathing, headach, or flying pains thro' the body; there are others, who, besides several of these symptoms in a higher degree, are also subject to uncommon hæmorrhages, faintings, and violent hysteric convulsions, on account of a greater delicacy and mobility of their nervous system.

AN obstruction or suppression of the *menfes* may produce nervous or hysteric disorders, either from the sympathy of the womb with the other parts, from a redundancy of blood, or from the retention of something hurtful to the nerves.

1. THAT many parts of the body may be affected thro' the consent of their nerves with those of the womb, will not appear improbable, after what has been said of the remarkable sympathy that takes place between the various parts of the body *. But it may be proper to observe, that when the *menfes* are obstructed, the stomach generally suffers first, and, by means of its consent with almost every part of the body, gives rise to many of the complaints which follow. Thus the hysteric convulsions, and other violent symptoms, which are sometimes occasioned by a sudden stoppage of the *menfes*, do not seem to proceed immediately from the *uterus*, but commonly from the stomach and bowels, whose nerves are first affected either by their sympathy with those of the womb, or by the blood which should have been discharged by this organ being partly turned upon the alimentary canal. For,

2. ALTHO' it is probable that the menstrual evacuation is not owing to a general *plethora* or increase of the mass of blood at the end of every month, but to the particular structure of the womb; yet as the body, after being long accustomed to any regular evacuation, seldom fails to suffer from a stoppage of it; so it is not to be doubted, that in women, and especially in the more sanguine, some degree of a *plethora* may be often the consequence of a suppression of the *menfes*. Agreeably to this we observe, that bleeding is often the

* See above, chap. I. No. II. (c).

the best remedy for the complaints incident to women at that time of life when their courses leave them.

3. If we consider, that, by means of the other excretory organs, some humour is thrown off, which if retained would prove noxious to the body, it will not appear altogether improbable, that the menstrual evacuation, when suppressed, may become hurtful by its quality as well as quantity: And this seems to be confirmed by those uncommon hæmorrhages from the eyes, ears, ends of the fingers, and other parts, upon the total suppression of the *menfes*; for such hæmorrhages cannot be owing to a general *plethora* occasioning too great a distension of the whole vascular system: were this the case, the vessels of the lungs and other internal parts must burst before any blood could pass by the pores of the skin. When one runs fast, or walks up a steep hill, the force of the blood is much more increased than it can be from any *plethora* that may be supposed to happen to women who are obstructed; and yet we never find that violent exercise makes the blood issue from the points of the fingers, or the pores of the *meatus auditorius*, altho' it sometimes occasions an hæmorrhage from the lungs. Further, if a general *plethora* were the cause of those uncommon hæmorrhages which happen in consequence of a suppression of the *menfes*, bleeding would always prevent them; which however it seldom does. An instance of this I had many years since in a patient, who, tho' she had lost by the lancet about forty ounces of blood in the space of a month; yet continued to have a small hæmorrhage from the left ear once in twelve or fourteen days. Another case no less remarkable, was that of a woman aged 34, who, near six weeks after bearing her second child, to which she gave suck, was seized with a pain in the middle of her fore-arm striking down to the middle finger of the left hand. Next day, she felt a pain in the point of that finger where there had been for two or three days a red spot, from which there issued about four ounces of blood. At the distance of 24 hours, she lost near an ounce of blood in the same manner; and notwithstanding she was blooded once and again, yet for some days, almost at the same hour, this hæmorrhage returned, but always in smaller

smaller quantity. In this woman the *lochia* had stopped soon after delivery.

If these periodical evacuations of blood cannot then be accounted for from a general *plethora*, is it not probable, that when the *menfes* are stopped, something hurtful may be retained, which falling on certain parts, in people whose nervous system is easily affected, may throw the small vessels of those parts into such violent alternate contractions, as to force the red blood, instead of the thinner fluids, thro' their dilated orifices? When in this manner the offending matter is mostly evacuated, the extraordinary motions of the small vessels, and consequently the flux of blood from them, will cease*.

In like manner, those various other complaints, consequent on the obstruction of the *menfes*, may be often owing to the quality of what is retained, which soon becoming acrid, disagreeably affects the nerves of those parts upon which it falls.

WHAT has been said of obstructed *menfes*, as the cause of nervous disorders, may be, in a great measure, applied to the suppression of the hæmorrhoids, in such as have been accustomed to them: And it may not be amiss, when treating of the suppression of the *menfes* and hæmorrhoids, to add the discharges of issues, setons, or other old sores suddenly dried up, as producing similar effects. Further, since cold feet, or cold and moisture in general, by stopping the perspiration, is observed to increase nervous disorders, is it not probable, that some acrid matter may be then retained, which, by falling on the stomach and other internal parts, sometimes gives rise to nervous, as well as to other morbid symptoms? Hence we find, that, during the dry warm weather in our climate, and the dry and temperate weather of hotter countries, the nervous, hysteric, and hypochondriac complaints are less frequent than at other times.

III. A third general occasional cause of nervous disorders may be, the want of a sufficient quantity of blood, or of blood of a proper density: And hence it is, that an immoderate flux of the *menfes*, *lochia*,

* See above, chapter 1. No. 17. and likewise p. 251. &c. where I have endeavoured to shew, by a variety of facts, that the small vessels, when affected with any unusual stimulus, are agitated with uncommon contractions.

chia, and hæmorrhoids, or any other great hæmorrhage, will often occasion violent symptoms of this kind.

HIPPOCRATES has observed, that convulsions may arise from innanition, as well as repletion: And, as the strength and firmness of the whole body depend upon proper fluids, and a due quantity of them, may not very irregular and strange disorders happen from a want of blood, or from a too watery state of it, especially in those whose nervous system is very delicate and easily affected? For, when there does not remain in the vessels a quantity of blood sufficient for carrying on the several functions in a proper manner, the regular circulation of all the fluids must be disturbed, and the distribution or exertion of the nervous moving power will become irregular.

BUT, in whatever way great loss of blood may give rise to nervous diseases, we are so certain of the fact from experience, that perhaps the following cases in proof of it may be thought superfluous.

1. A young gentleman of 17 years of age, complaining of a pain in his right side after a fall from his horse, was bled very largely. Some days after, he felt a coldness in his stomach, which was soon succeeded by fits of violent pain and spasms in that part, sometimes lasting twenty minutes, or half an hour, at a time. These symptoms returned after no regular intervals, but generally twice, or oftener, in 24 hours; and gradually increased to such a height, that the patient was obliged to be held down in bed by two or three people, in order to prevent his tearing his hair, and doing himself other mischief. The pain and cramps were always preceded with a sensation of coldness in the stomach, and frequently went off in an instant. Ginger with hot brandy felt cold in the stomach at the accession of the fit. After the patient had suffered in this manner for three weeks, the disorder gradually abated; and, by using a few stomachic medicines, a proper diet, and exercise, he perfectly recovered.

ON another occasion, after losing a good deal of blood, he was attacked with the same symptoms, but in a much less violent degree.

2. A gentleman between 40 and 50 years of age, has been subject for these 13 years past, to a considerable flux of pale urine, especially in the night, which has sometimes continued for two or three weeks, and has not been stopped without using large quantities of the bark, riding, and other remedies; and this has generally happened to him, as often as he has been obliged to be blooded to any considerable quantity, or to have sweated much, and at the same time to have lived low for a few days, in order to get rid of a cold, rheumatism, or other casual disorder.

3. A gentlewoman in whom the *menfes* flow too plentifully, is frequently troubled with a gnawing pain, and sometimes a flatulent distension of her stomach, when she is not with child; but, during the time of pregnancy, she is generally free of any such complaints.

4. IN the Philosophical Transactions, No. 174. we have a remarkable case by Dr Cole, of a lady subject to hysterics, who, after being much reduced by an uncommon loss of blood in child-bearing, was for a long time affected with violent periodic convulsions, accompanied with a great flux of limpid urine, returning every fourth or fifth day at a certain hour.

UNDER this head of general occasional causes, may also be comprehended watching, great fatigue, and excessive venery; all of which not only tend to break the constitution, and dispose the body to nervous diseases, but also to create them, especially in such as are already predisposed to them.

HAVING thus far treated of the general occasional causes, we shall proceed next to mention the chief of those occasional causes which we call *particular*, from their having their seat in certain parts of the body.

C H A P. V.

Of the particular occasional CAUSES of NERVOUS, HYPOCHONDRIAC, and HYSTERIC Disorders.

THESE may be reduced to the six following, *viz.*

- I. Wind
- II. A tough phlegm
- III. Worms
- IV. Aliments improper in their quantity or quality.
- V. Scirrhus or other obstructions in the *viscera* of the lower belly.
- VI. Violent affections of the mind.

I. WIND in the stomach and intestines, tho' of itself a very common symptom in nervous disorders, yet deserves a place among their occasional causes, as giving rise to many uneasy sensations. Altho' all our food abounds more or less with air, yet in the time of digestion, it is seldom separated in such a quantity as to give any trouble, unless when the stomach and bowels are weak, or when their nerves are endued with an uncommon sensibility: But in such circumstances, the complaints it occasions are various, such as want of appetite, *nausea*, faintness, low spirits, watching, swelling of the stomach and bowels, violent pains in them, tightness, and oppression about the *præcordia*, difficult breathing, a sensation of a weight in the stomach, belching, the *globus hystericus*, giddiness, shooting pains in the head, &c. Nay, I have frequently felt in myself, a plain connection between wind in the *primæ viæ* and pains in my legs

legs and feet; and the uneasy sensation sometimes, as it were, coming and going between these parts.

THE manner in which wind produces so many and such various complaints, may be understood from its distending the stomach and intestines, and thereby occasioning spasms in those parts, or otherwise disagreeably affecting their nerves, which have so great a sympathy with the other parts of the body*.

HOWEVER, it may be proper to observe, that the effects of wind in the first passages are not only various in different persons, but in the same person at different times. In people whose stomach and bowels are in a sound state, if wind happens to be collected, it may create some uneasiness, but does not quicken their pulse, or affect them with that disagreeable sensation, anxiety about the *præcordia*, or depression of spirits, so often its consequences in those whose alimentary canal is endued with a more delicate feeling.

4 C 2

Nay,

* See above, chap. I. No. 11.

Some have imagined, that the flatulence produced in the stomach and bowels, passes freely by means of the absorbent veins into the blood, with which it circulates through the body, and produces a variety of symptoms, such as shooting pains in the head, the *clavus hyssericus*, or flying pains in the arms, legs, and other parts, palpitations of the heart, a fluttering motion of some of the fibres of the voluntary muscles, and puffy swellings below the skin. At other times, when these complaints cease, and the first passages suffer more from wind, they suppose, that the flatulence finds a ready way from the blood into the stomach and intestines by their pores or exhaling arteries. This opinion, however, is ill founded; for experiments made on animals newly dead shew, that neither the stomach nor intestines, nor even the *peritonæum*, which is much thinner, are pervious to elastic air: and we know, from other experiments, that capillary tubes, or absorbent vessels, do not attract elastic air as they do watery fluids; nay, small portions of air, when they get into such tubes, prevent their attracting any more of other fluids.

In hypochondriac and hysteric patients, I have observed little swellings or elevations of the skin, of a pale colour, and of different shapes. These, in a few minutes, acquired their full size, and after half an hour or more would quickly vanish. In hysteric women also, we meet with soft puffy swellings below the skin, which, because of their sudden rising and disappearance, have by some been ascribed to wind shifting from one part of the cellular membrane to another. But this is no ways probable: and both these puffy swellings, and those risings of the skin, seem to be owing to the same cause, *viz.* an increased alternate motion of the small arteries of the parts, occasioned by an uncommon irritation of them or their nerves; whence there must happen an effusion of a serous or lymphatic fluid in the spaces of the *tela cellulosa*, or in the interstices of the skin, which, as soon as the extraordinary motion of the small vessels ceases, will be quickly absorbed; and consequently those swellings will disappear.

Nay, the same persons are, at different times, very differently affected by wind, just as the nerves of the stomach and intestines happen to be more or less sensible, or their feeling more or less different from what it is in a natural state. Thus, when an arthritic or rheumatic humour in the blood is turned upon those *viscera*, the wind produces a much more uneasy sensation than at other times.

FURTHER, the great distension of the intestines, and sometimes also of the stomach, in a tympany, without those uneasy complaints that attend wind in hypochondriac or hysterical cases, shews, that, unless there be a particular indisposition of the nerves of these organs, flatulence alone will not give very remarkable disturbance.

II. A tough phlegm in the stomach and intestines.

PATIENTS generally imagine, that this is produced by their food, which they believe is all turned into phlegm: But they are mistaken; for while the stomach remains disordered, be the aliments ever so little of a glutinous nature, this substance will be continually generated.

IN the alimentary canal, besides the fine exhaling arteries, which furnish the gastric and intestinal lymph, there are many small glands, which secrete a liquor of a more glutinous nature. In a sound state, this *mucus* is in no greater quantity than what is necessary to defend the delicate nerves of those parts from the heat, cold, acrimony, or attrition of the food; but when the secreting vessels have lost their tone, or are affected with an unnatural *stimulus*, not only the mucous glands, but also the exhaling arteries, may throw out, in a greater quantity, a viscid fluid, which, by lying some time, may acquire still a greater degree of cohesion.

WHEN much phlegm is collected in the stomach and intestines, their nerves are rendered less sensible of the *stimulus* of the aliments, their absorbent vessels are partly obstructed, and the gastric and intestinal lymph are more sparingly secreted, or, at least, become more viscid. Hence, the digestion and absorption of the finer parts of the food are, in a great measure, prevented; whilst this phlegm, by disagreeably affecting the nerves of the alimentary canal,

canal, especially when they are in a delicate state, occasions want of appetite, sometimes an unnatural craving for food, a *nausea*, flatulence, gripes, and looseness, cold and hot fits, a quick pulse, weakness, faintings, lowness of spirits, sleepiness, sighing, convulsive motions *, and giddiness. Nay, I have had some patients who, from a viscid phlegm in their stomach, were affected with a slight *delirium*, and had their eyes like those of people in liquor.

NOR will it appear strange, that so many and such different symptoms should proceed from a disorder in the stomach and bowels only, if we attend to that sympathy which I have so often mentioned, as taking place between them and the other parts of the body.

III. WORMS in the first passages, especially in children, are frequently the cause of nervous symptoms, such as great craving for food, inflations of the alimentary canal, hiccup, vomiting, dry cough, difficult breathing, sighing, irregularities of the pulse, palpitations, tremors, convulsions, epileptic fits, drowsiness, raving, insensibility, &c.

WORMS produce most of these symptoms, by preventing the proper digestion of the food, or by irritating, with their frequent motions or biting, the sensible nerves of the stomach or bowels, whence every other part may be affected by sympathy.

SEVERAL of the above symptoms may also be occasioned by acrid humours in the *primæ viæ*; as will appear by the following case.

A BOY of 14 years of age, on the 12th of January 1757, was seized with a pain in his head and belly, and soon after became delirious, and made no answer when spoke to. When awake, he sometimes cried out in a wild manner, as if complaining, or praying to be freed from his trouble; but his words had generally little connection. He slept well, had a sharp appetite, was not costive, and

* A girl aged 14, who had been troubled with the *chorea Sancti Viti*, was seized with the measles. A few days after her recovery, she had a return of her former distemper, which, after it had continued near a fortnight with little abatement, notwithstanding the use of several medicines, was entirely removed in a few days by a natural looseness, by which she voided a great deal of slimy stuff. It may be worth remarking, that, during the continuance of this convulsive disorder, her appetite was much greater than usual.

and his pulse was full and flow, but somewhat irregular. These symptoms continued till the 16th of January, when I saw him first, and ordered seven ounces of blood to be taken away, a blister to be applied between his shoulders, and a clyster to be injected. On the 17th, no better: the blister had occasioned a strangury. On the 18th, took a bolus of calomel and rhubarb, but soon vomited it up again. On the 19th, swallowed five grains of calomel at bed-time, and next morning had three stools, after which he became much more sensible, but still complained of his head: 21st, had a natural stool in which were two small worms of the *ascarides* kind. Upon this he was ordered *pulvis stanni*, and another dose of calomel and rhubarb, which brought away a great deal of slime, but no worms. On the 25th he was free from all his complaints.

THIS patient in July 1758, having had a return of the symptoms above mentioned, he was blooded without any benefit; but was greatly relieved by a dose of rhubarb and calomel, and entirely cured by a repetition of it, altho' no worms were found in his stools. At this time, as well as in his former illness, he had a greater appetite than usual, especially when the disease began to yield.

IV. ALIMENTS improper in their quantity or quality.

THE most wholesome food, in too great a quantity, oppresses the stomach and bowels, is not properly digested, but becomes either acid or putrid, and generates much wind; whence the nerves of those parts being disagreeably affected, a variety of complaints are produced.

ON the other hand, the want of a due quantity of aliments occasions faintness and wind, and, in time, so much weakens the stomach and bowels, as to render them unfit either to receive or to digest what is necessary for supporting the body.

BUT altho' food be taken with neither too full nor too sparing a hand, yet its quality may dispose it to produce nervous disorders. Thus high-seasoned and heavy meats, strong fauces, and wines, will not only, by degrees, enervate the tone of the stomach, and prevent

vent or destroy the natural feeling of its nerves, but will corrupt the blood, perhaps breed the arthritic matter, and bring on a diseased state of the whole body. On the contrary, a watery and flatulent diet, by disagreeably affecting the nerves of the first passages, generating a great deal of wind, and not affording proper nourishment, will be the cause of many ailments.

It is, however, to be observed, that aliments, either hurtful in their nature or quantity, will chiefly produce nervous symptoms in those, who, from the peculiar state of their alimentary canal, are most liable to such disorders.

Thus, wind or crudities in the *primæ viæ*, occasioned by diet, will often give no great uneasiness to those of firm nerves, and whose stomach and bowels are strong; but in more delicate people, on account of the particular sensibility of these organs, such causes will either excite painful spasms, or other disagreeable sensations, attended with lowness of spirits.

I observed above, that in some the stomach becomes so very delicate, that even a sudden change of posture will be apt to occasion a *nausea* or vomiting: and there are others, who, when their stomach is empty, especially after a late error in diet, feel an uneasy craving, faintness, and giddiness, which symptoms are almost as certainly relieved by a little solid food, or a glass of wine, as pain is by *opium*. To this faintness and disagreeable sensation in the stomach, when empty, those are most liable who, besides a particular weakness of that organ, carry an arthritic matter in their blood frequently affecting it.

V. SCIRRHOUS, or other obstructions in the stomach, intestines, liver, spleen, pancreas, mesentery, *uterus*, and *ovaria*, often produce symptoms of the hypochondriac or hysteric kind; such as want of appetite, *nausea*, cramps in the stomach, vomiting, sometimes of a black or bloody coloured matter, flatulence, and crudities in the first passages, hectic heats, cold sweats, low spirits, and other complaints, more or less violent, according as the patient's nerves are more or less delicate.

SUCH

SUCH obstructions in the stomach and bowels seem to occasion many of the above effects, by hindering the free circulation of the fluids through these parts, by affecting their nerves with an uneasy sensation, and by preventing digestion. In the liver and spleen, by impeding the secretion of bile, and, by their weight, occasioning a disagreeable sensation, not only in these, but the neighbouring parts by sympathy *. In the mesentery, by preventing the further preparation of the chyle, and its course towards the thoracic duct. In the *uterus* and *ovaria*, by disturbing the functions of these parts, and by consent affecting the stomach and bowels. Further, hard swellings in the *uterus*, or other abdominal *viscera*, by irritating such nerves as are contiguous to them more at one time than another, may give rise to spasmodic contractions of the intestines in some parts, and flatulent distensions of them in others, and may so affect the whole nervous system as to occasion hysteric faintings and convulsions.

As obstructions in the stomach, liver, &c. may be often the cause of low spirits; so, on the other hand, melancholy, or long continued grief, frequently gives rise to hypochondriac and hysteric complaints, and sometimes to obstructions in those *viscera*. For such a state of the mind not only disorders the nerves of the stomach, liver, and bowels, and occasions a want of appetite and digestion, with its various consequences; but, by means of the agency of those nerves, it may also produce, in some of the small vessels of these *viscera*, such a fixed spasmodic contraction, as to lay the foundation of an irresolvable obstruction; in much the same manner as a sudden fright has given rise to a *scirrhus*, and afterwards to a cancer in the breast. Further, the slow interrupted breathing, and the sedentary life of those who are much affected with grief, will make the fluids more apt to stagnate, and consequently to form obstructions in the small vessels of the hypochondriac *viscera*.

I. A

* In the bodies of those who have died of the hypochondriac disease, the meseraic, and other veins which meet to form the *vena portarum*, have been often found greatly distended with blood. But this distension of those veins, if any thing preternatural, was probably only a consequence of some obstruction in the liver, and not to be reckoned, as it has been by some authors, the cause of that distemper.

1. A gentleman aged 60, who had been above three years subject to cramps and pains in his stomach, want of appetite, belching, fits of sickness and vomiting, began, in spring 1748, to throw up a dark-coloured liquor like coffee-grounds, and to void the same by stool. In the end of April 1749, he vomited a greater quantity of this black stuff than ever, and soon after he threw up about an English quart of blood, mostly clotted, which reduced him so low, that he never recovered his flesh or colour. Throughout the summer, he continued in a declining way, being much oppressed with belching, sickness at the stomach, and frequent retchings to vomit, tho' rarely bringing up any thing, but a tough phlegm, till the beginning of October, when, after severe sickness, he one morning vomited a great deal of blackish coloured stuff, and in the evening a considerable quantity of clotted blood. On the 15th of this month, about eleven in the forenoon, after retching to vomit, he complained suddenly of a sharp pain below the false ribs of his left side: immediately after which, his pulse began to sink, and he died at two o'clock.

HIS body being opened, the coats of the stomach were found thick and scirrhus in several parts, especially about its left orifice. In those morbid parts several small ulcerations and chops were observed, and near the bottom of the stomach a hole as broad as a shilling. This part, which had been thinner than the rest of the stomach, seems to have given way on the morning before the patient died, and the laceration was probably the cause of that sharp pain he complained of in his left side. There was nothing in the stomach, as all its contents had been emptied into the cavity of the *abdomen*.

IT can hardly be doubted, that the black-coloured liquor, which this patient frequently vomited, as well as the clotted blood, came from the vessels of those scirrhus parts of the stomach, in which the small ulcerations and chops were observed. The blood that oozes slowly into the stomach from very small vessels, may lie for a considerable time before it is thrown up, and acquire a dark brown or blackish colour; but when it flows in greater quantity, and from

larger vessels, it is vomited up, either partly coagulated, or quite fluid, if it has remained only a very little time in the stomach.

It may be proper to observe, that the black as well as bloody vomitings were probably increased, if not first occasioned by the frequent emetics which the patient had taken to remove the sickness, want of appetite, and other complaints of his stomach: and undoubtedly whenever there is a confirmed *scirrhus* in the stomach, strong vomits must increase, exasperate, or inflame it, and probably break some of the vessels leading to the tumor. In such cases therefore, instead of *ipacuanha* and antimonials, the patients should use nothing but warm water, or a decoction of camomile-flowers, which will be sufficient to relieve the stomach when foul, without occasioning such violent convulsive contractions in it as the stronger emetics do.

2. A maiden gentlewoman about 30 years of age, in September 1755, began to complain of want of appetite, and wind in her stomach, and lost her flesh and strength. From the beginning of March following, her pulse became quicker than natural, and she then began to bring up every thing she swallowed, two or three hours after; and seldom went to stool without a clyster. When her stomach was empty of victuals, she threw up tough phlegm, which, a few days before her death, was mixed with some blackish matter. She never complained of any acute pain, but only of an uneasiness and tightness about the stomach. Her bowels were much distended with wind, which gave her a great deal of trouble; and the air shifting frequently from one place to another, produced considerable swellings, which could be easily felt outwardly. After trying various medicines to little purpose, she died about the end of May.

UPON opening her body, the *colon* was observed to be much contracted in several places, and, in the right side, to adhere to the *peritonæum*; but the chief cause of her complaints and death appeared to be a scirrhus tumor, which spread over the whole *pylorus*, and a small part of the stomach adjoining to it. The sides of the *pylorus* consisted of a firm cartilaginous substance, near an inch thick, and the passage was so straitened as scarcely to admit a quill.

quill. On the inside of the *pylorus* were found some small chops and inequalities, from which, as I imagine, a considerable part of the phlegm which she vomited might come. Be that as it will, it is scarce to be doubted, that the black-coloured stuff was furnished by the mouths of the small blood-vessels in those ruptured parts of the *pylorus*. If these vessels had been larger, this matter would have had more of a dark brown, or redish colour; or blood itself, either fluid or clotted, would have sometimes appeared. At no rate could this matter come from the liver; for that part was found: nor, supposing it otherwise, could any thing have easily passed from the *duodenum* into the stomach, on account of the straitness of the *pylorus*.

3. A female child, which from its birth had been afflicted with wind, gripes, and violent convulsions, died at the age of five months, after many remedies had been used unsuccessfully. Nothing preternatural was discovered upon dissection, except a portion of the *colon*, about five inches in length, quite scirrhus.

4. A gentlewoman, who had born several children, and had been generally healthy, in the 59th year of her age, ten years after the *menfes* had left her, began to complain of pains in her back, groins, and belly, above the *os pubis*, the violence of which brought on the *fluor albus*, and frequently a discharge of blood from the *vagina*. These pains lasted usually five or six hours, and returned every day nearly at the same time. During the fit she had always this hæmorrhage; but at other times the white flux only.

NOTWITHSTANDING the use of several medicines for twelve or fourteen months, her complaints were increased; the pains which now began in her legs and thighs, and rose to the lower parts of her belly, returned regularly every morning at ten, and were so acute, that she cried out almost the whole time they lasted; nor had she now any perfect intervals of ease. During the paroxysm, her pulse was small and quick, and her body cold, altho' all over in a sweat. Her pains were always most severe and lasting when she was costive, which happened often. The matter discharged from the *vagina* had no offensive smell. She was much troubled with wind in her

stomach and bowels. While the fit lasted, she never made any water, but spit a great deal more than usual. By the continuance of her disease, she gradually wasted away, and at last died.

As I was only consulted for this person at a distance, I never learned whether her body was opened or not; but I think there can be little doubt, that almost all her complaints, and particularly the sharp periodic pains in the hypogastric region, were owing to a *scirrhus* in the *uterus* beginning to turn cancerous.

VI. VIOLENT affections of the mind.

NOTHING produces more sudden or surprising changes in the body, than violent affections of the mind, whether these be excited by external objects, or by the exercise of the internal senses. Thus doleful or moving stories, horrible or unexpected sights*, great grief, anger, terror, and other passions, frequently occasion the most sudden and violent nervous symptoms. The strong impressions made in such cases on the brain and nerves, often throw the person into hysteric fits, either of the convulsive or fainting kind. Long continued grief and anxiety of mind weaken the tone of the stomach, destroy the appetite and digestion, occasion thirst, a white tongue, flatulence, and other complaints†. Great fear produces paleness of the countenance, an universal debility and shaking, palpitations of the heart, anxiety about the breast, quick breathing, and a looseness, or a large discharge of limpid urine. By sudden terror, delicate women or children have been not only thrown into fainting and convulsions, but rendered subject all their lifetime to epileptic fits. Anger quickens the pulse and respiration,

and

* It is said, that the great Lord Verulam was wont to faint, when he saw an eclipse of the sun: and we are told of a Lady, who, upon looking through a telescope at the comet of 1681, was struck with such terror, that she died in a few days. Pechlin, *Observat. med.* lib. 3. observ. 23.

† “Qui laborant animi pathemate, potissimum corripi solent morbis ventriculi, ut, inter caetera, observavi in moerentibus, qui conqueruntur primo de languore ventriculi, mox inappetentia, oris amaritie, siti circa horas matutinas, cruditatibus, flatibus, et tensionibus hypochondriorum.” Baglivii opera, 4to, p. 565.

and increases the force of the heart: Hence it has been immediately followed by an uncommon excretion of the *saliva*, by bilious vomitings *, bleeding at the nipples †, and a rupture of such vessels as were lately cicatrized. In women it frequently occasions spasmodic contractions in the bowels, and a flatulent or hysteric cholic. Some of the more violent passions have, all at once, occasioned a kind of *tetanus*, or catalepsy; so that the person has appeared liker to a statue than to any thing alive: nay, excessive fear, grief, joy, and shame have been sometimes followed by sudden death.

BONETUS has recorded the case of a lady, who, among other hysteric symptoms, owing to grief and disappointments, was seized with frequent fainting fits, which sometimes lasted half an hour ‡: And several years ago, I had a patient, who, upon the unexpected death of her husband, fell into such fits, generally holding her from five to fifteen minutes. In these faintings she lay like a dead person, without any apparent breathing or motion of the breast; only, when a candle was held near her mouth, the flame was observed to move a little. Her pulse however was scarce changed, only somewhat slower and feebler than usual. She came out of these faintings with sighings and crying, and generally relapsed into them in little more than a quarter of an hour. In this way she continued for two days.

BAGLIVIVUS mentions a young man of Dalmatia, who, from looking at a person in an epileptic fit, was himself affected in the same manner ||: And it has frequently happened in the Royal Infirmary here, that women have been seized with hysteric fits, from seeing others attacked with them. But one of the most remarkable instances of this kind happened in the poor's house at Haerlem, in the time of the learned Dr Boerhaave, and is recorded by his nephew in the following manner.

“IN domo, qua pauperes ex eleemosynis publice aluntur in civitate

* Pechlin. lib. 3. observat. 25.

† Stalpart. Vander Wiel. cent. 1. obs. 74.

‡ Sepulchret. anatom. lib. 2. § 33. obs. 9.

|| Praxis Medica, cap. 14. § 2. See also Natur. curios. 1730, p. 302.

“vitate Harlemensi, perterrita puella incidit in morbum nervo-
 “rum convulsivum, certis paroxysmis reducem: Adstantium et ad-
 “juvantium in eam intenta itidem corripitur eodem morbo; postri-
 “die altera, deinde tertia, quarta, imo fere omnes, tam pueri quam
 “puellæ: Status miserrimus! Corripitur hic, corripitur illa, imo
 “fere omnes eodem tempore, dum unum alter aspicit, prosternun-
 “tur. Medici solertes frustra adhibent quæ dictat ars, saluberri-
 “rima antiepileptica medicamina. Confugitur tandem ad Boer-
 “haavium, qui, misertus infelicis pauperum fortis, petiit Harle-
 “mum, et dum rem examinat, invadente in unum paroxysmo, vi-
 “dit convelli plures specie epilepsiæ. Datis incassum optimis reme-
 “diis a medicis sapientibus, et ad imaginationem ex uno in alte-
 “rum traducto morbo, rite perpenſis, hanc avertendo, credidit posse
 “curam obtineri, et obtinuit. Scilicet, praemonitis ephoris, praesen-
 “tibus omnibus, jussit per cameram disponi fornaces portatiles,
 “prunis ardentibus instructas, atque iis imponi ferreos hamulos,
 “ad certam figuram adaptatos; tum ita mandavit: Quia omnia
 “frustra forent, se aliud nescire remedium, quam, ut qui primus,
 “puer foret vel puella, infausto morbi paroxysmo arripiretur, lo-
 “cus quidam nudati brachii candente ferro ad os usque inurere-
 “tur; utque gravitate pollebat dicendi, perterriti omnes ad cru-
 “dele remedium, dum instare sentiunt paroxysmum, omni mentis
 “intentione, et metu dolorificæ inustionis, eidem resistunt fortio-
 “ris oblatione ideæ: et certe quantum valeat hic ab objecto ani-
 “mæ intentæ revulsio, docet epilepsia diversimode curata, ut qui-
 “dem ipse terror eandem sustulerit, febris epidemica, quartana,
 “ptyalismus, matrimonium, virga *.”

THERE is a disease very common in the island of Zetland, which
 is known there by the name of the *convulsive fits*. It begins with
 a violent palpitation of the heart; soon after which the patients fall
 to the ground, unless they are supported; their arms and legs are
 alternately contracted and relaxed; and, in some cases, their joints
 become so rigid that they cannot be bent. Their respiration seems
 to

* Abr. Kaau Boerhaave Impet. faciens Hippocrati dictum, § 406.

to be difficult, and they cry terribly while the fit lasts, which is generally less than a quarter of an hour; altho', in some rare cases, it has continued above an hour. This disorder seldom attacks married women; but young women, and even girls of twelve or ten years of age, are liable to it. Some boys and two young men in the island have been also affected with it. In the church or other public meetings, as soon as any one is seized, all such as have been formerly subject to the distemper are attacked with it, which often occasions great disturbance; and some who never had these fits have been affected, upon seeing or even hearing the noise of such as are seized with them.

THIS disease does not seem to impair the health of the patients; for the young women subject to it are generally as strong, and, in other respects, as healthy as any in the island.

WE have seen above*, that there is a remarkable sympathy, by means of the nerves, between the various parts of the body; and now it appears that there is a still more wonderful sympathy between the nervous systems of different persons, whence various motions and morbid symptoms are often transferred from one to another, without any corporeal contact or infection.

IN these cases the impression made upon the mind, or *sensorium commune*, by seeing others in a disordered state, raises, by means of the nerves, such motions or changes in certain parts of the body, as to produce similar affections in them: And hence it is, that the sight only of a person vomiting, has often excited the same action in others; that sore eyes become sometimes infectious; that yawning is propagated from one person thro' a whole company; and that convulsive disorders are caught by looking on those who are affected with them. Now, altho' we cannot explain how different impressions made on the *sensorium commune* should occasion, by means of the nerves, those various changes in the body; yet that the nerves are really capable of producing very sudden changes in the circulation and distribution of the fluids, when the mind is variously affected, we have full proof in that redness of the face which accompanies

* See chap. 1. No. 10. and 11.

accompanies a sense of shame, that increased flux of the *saliva* which happens to a hungry person upon the sight of grateful food, and that plentiful discharge of tears which is often produced by piteous objects or tragical stories.

THUS far we know, from certain experience, that when the nervous system is extremely delicate, a small impression on any of the organs of sense will often throw the whole body into disorder. For example, I have known people of weak nerves, subject to wind in their stomach, and a swimming of their head, who, by looking into a mirror that was kept constantly moving before them, became so giddy, as to be in hazard of falling. Others, upon the sudden opening of a door, or any other unexpected noise, have been liable to be seized with convulsions. Nay, there have some, whose brain and organs of sense were so susceptible of impressions, that they could scarce abstain from imitating every motion and gesture they saw performed by others*.

ON the other hand, it is to be observed, that strong nervous symptoms are seldom occasioned by fear, terror, grief, the force of imagination, or any sudden impression on the organs of sense, in persons whose nerves are firm and less sensible; but, when the contrary is the case, the causes above mentioned will often produce the most sudden and violent hysteric fits, or convulsive disorders, without any fault in the womb, alimentary canal, or other parts of the body.

To conclude our observations on the causes of nervous diseases, it may be proper to take notice, that although it appears from the dissections of those who have died of them, that the stomach and intestines, liver, spleen, *omentum*, mesentery, or *uterus*, have frequently been found obstructed, scirrhus, or otherwise unsound; yet, as in many other cases of the same disorders, no such morbid appearances have been observed in the body after death; it follows, that these symptoms may frequently proceed from causes which, eluding our senses, are not to be discovered by dissection. Nay, obstructions, *scirrhi*, and other disorders of the *viscera*, observed in those

* Philosophical transact. abridg. vol. 3. p. 8.

those who have died after suffering long from nervous ailments, seem sometimes to have been the consequences of a long state of bad health, rather than the causes of it: Particularly, by the frequent attacks of that arthritic or other morbid matter, which is often the cause of these disorders; and, by the pains and spasms attending them, some obstructions may at length be formed in the small vessels of the stomach or neighbouring parts; to which every new return adds a little, just as new inflammations of the *cornea* always increase specks on that membrane.

C H A P. VI.

Observations on some of the most remarkable symptoms of the
NERVOUS, HYPOCHONDRIAC, and HYSTERIC kind.

I. **A**N uncommon sense of cold or heat in different parts of the body, sometimes suddenly succeeding each other.

THE natural heat of animal bodies is owing to the regular and uninterrupted circulation of the fluids. As this degree of heat, however, is nearly the same in every part that is defended from the external cold, and is what we are accustomed to, we are commonly no more conscious of it than of the beating of the heart, or of the alternate contraction of the intestines. But as often as there is a more rapid motion of the fluids through the whole body, or only in the smaller vessels of some part, we feel a greater heat than the natural. In like manner, a sensation of cold proceeds from a diminished circulation, or a stagnation of the fluids in the smaller vessels.

IN hypochondriac and hysteric cases, a quicker or slower motion of the fluids, and consequently an unusual sensation of heat or cold in the vessels of the head, back, arms, legs, and other parts, may arise either from the vessels themselves, or their fluids. From the vessels, when these, from some fault or irritation of the nervous system, or from sympathy between their nerves and those of the stomach, or some other very sensible part, are either thrown into an unusual alternate motion, or affected with a continued spasmodic stricture: From the fluids, when, by their acrimony or viscid quality, the very small vessels are either excited into un-
common

common vibratory contractions *, or become in a great measure obstructed.

It is observable, that in those parts of the body, in which patients complain of an unusual heat or cold, we can often, neither by our feeling nor the thermometer, discover a greater or less degree of heat than in the neighbouring parts, where there is no such sensation. This may be owing to the heat or cold in such cases being felt below the skin and *membrana adiposa*, viz. in the muscles: or perhaps a violent alternate motion excited in the very small vessels by some acrid fluid may give a fallacious sense of heat to the person, when there is really no greater degree of it, as far as can be discovered by the thermometer. In like manner, the stagnation of some viscid humour in the same vessels may produce a sense of cold, although the real heat is little, if at all, diminished. 'Tis true, that in blushing the increased motion of the fluids thro' the vessels of the face is accompanied with a glow; but it is to be considered that the increased motion here is in the vessels of the skin, and such also as carry red blood, which seems more apt to acquire heat by friction or agitation than the thinner fluids.

THE sense of cold and shivering in the beginning of most fevers and inflammations, seems not to be owing, as some have imagined, to viscid fluids stagnating in the small cutaneous vessels, but to a spasmodic contraction of these vessels, in consequence of that irritation which the nervous system suffers from the febrile *stimulus*, or the beginning inflammation. However, altho' all fevers proceed from irritation, and consequently from an affection of the nerves, and many fevers of the low kind have had the name of *nervous* peculiarly bestowed on them; yet a regular intermittent seems to deserve that appellation better than almost any other species of fever; for its paroxysms, like those of the epilepsy, or other convulsive disorders, are owing, not so much to any fixed obstruction in the vascular system, or general acrimony or

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viscosity

* That acrid humours and affections of the brain and nerves may excite a violent alternate motion in the small vessels, or affect them with a continued spasm or *tetanus*, we have shewn above, chapter 1. No. 17.

visciduity of the mass of fluids, as to an irritation or affection of the nerves of some particular part, such as the stomach or intestines; whence the whole system suffers by sympathy, and a shivering is produced, which is succeeded by a hot fit and sweating, that for the time removes the cause of the disease. And, as an intermittent agrees with epileptic and other convulsive disorders as to its cause, so its returning paroxysms, like theirs, may be often prevented or weakened, by raising, a short time before the approach of the fit, an acute pain or any great commotion in the body.

II. PAINS in different parts of the body, suddenly moving from one place to another.

THESE pains are something a-kin to those of the rheumatic kind, but generally have their seat in the skin, membranes, and muscles, and not in the ligaments and joints. Their shifting suddenly, and their seeming sometimes to have a connection with the flatulent complaints of the stomach, has induced some physicians, as well as the patient, to ascribe them to the motion of the air between the skin and muscles, from one part of the body to another. But their true cause seems to be, either some viscid or acrid matter sticking, for a short time, in the small vessels of certain parts, and irritating them, or spasmodic contractions of these vessels from a sympathy between their nerves and those of the stomach and intestines, or some other very sensible part*.

FROM the same causes affecting the vessels or nerves of the *pericranium*, or other parts of the head, proceed flying pains in this part, and the *clavus hystericus*, which Sydenham, who imagined the hysteric disease to proceed from a confusion of the animal spirits, ascribed to the whole spirits of the body being contracted into a small part of the head, and producing much the same sensation as if a nail were driven into it†.

THAT

* It is observable, that Diocles Carystius mentions, among the signs of disorders of the belly, pains flying through the body, without any apparent cause. Vid. Epist. ad Regem Antigonum.

† Sydenhami Opera, epist. ad D. Cole.

THAT those pains in the head often proceed from a sympathy with the stomach, is rendered probable by the violent vomiting which sometimes accompanies the *clavus hystericus*, and by observing, that people much troubled with wind in their stomach, and flying pains in their head, are not so often affected with these pains when they are free from the flatulence.

III. HYSTERIC faintings and convulsions.

MANY hysteric women are liable to be seized with faintings, during which they lie as in a deep sleep; only their respiration is so low as scarce to be perceived. Others, along with faintings of this kind, are affected with catchings and strong convulsions.

THESE fits come on differently in different patients; in some a coldness, attended with a sense of stiffness, is first perceived in the legs, or in the trunk of the body; after this a yawning and stretching of their arms; a lowness of spirits, with an oppression about the *præcordia*; the stomach or some part of the intestines is distended with wind; they often feel, as it were, a ball in their throat; their breathing becomes quick; the heart flutters, or is affected with a strong palpitation; a giddiness, a noise in the ears, and a loss of sight, as well as of the other senses, succeed, together with convulsive motions of the extremities and other parts of the body.

FITS of this kind may be owing to various causes: Such as;

1. AN irritation of the nerves of the stomach or intestines, from wind, acrid humours, or other causes, whence the whole system is often brought into consent. Nor can it admit of any doubt, that hysteric fits frequently proceed from this cause; since the patients are often sensible of their beginning with an uneasy sensation in those parts.

2. A sudden suppression of the *menfes* often gives rise to hysteric fits: And in some a fatal apoplexy, attended with a violent spasm of the muscles of the *glottis*, has been the consequence of the menstrual evacuation being suddenly stopt; as in the following case.

AN

AN unmarried woman, aged 20, of a delicate habit, having exposed herself to cold at the return of the monthly period, was, next morning at four o'clock, suddenly seized with a *stupor*, and a difficulty in speaking and moving her limbs. She was soon after blooded, and a blister was applied between her shoulders. At eight, when I first saw her, she could neither speak nor swallow; she then laboured under a hiccup, her face was pale, her skin cold, altho' her pulse and breathing were natural. About half an hour after ten, she began to breathe with labour, and with a snorting noise. This struggle, however, especially in expiration, did not arise from any fault in the lungs or muscles of respiration, but from a spasm of those muscles of the *larynx* which shut the *glottis*; and it came by fits, which continued three or four minutes, and sometimes more. In the intervals, which were somewhat longer than the accessions, she breathed pretty easily. The *oleum succini* held to the nose lessened at first the spasmodic contraction of the *glottis*, and made her breathe easier. She was blooded again, and had a purging clyster injected. About eleven, the fore-part of her neck around the *larynx*, and under the sterno-mastoid muscles, was much swelled, as if the cellular membrane had been distended with air. A poultice of *theriaca* and camphire applied to this swelling seemed to lessen the violence of the fits of difficult respiration. In the afternoon, her pulse becoming quick and full, and her skin hot, she was blooded a third time; but, notwithstanding this and other remedies, she died that night at ten; eighteen hours after she was first taken ill.

3. A very acute pain in any of the more sensible parts of the body, or violent affections of the mind, as terror, grief, anger, or disappointments, will sometimes so strongly affect the whole nervous system, as to bring on hysteric faintings, with convulsions, altho' the body be in every respect healthful and sound, bating the too great delicacy or sensibility of the brain and nerves.

IV. A *Catalepsis* and *Tenatus*.

OF all the nervous or spasmodic disorders, there is none more surprising

surprising than the *catalepsis* or *stupor vigilans*, as it is called by Fernelius *. In this the patient becomes either wholly, or in a great measure insensible of what is doing about him, and remains exactly in the same posture in which he was first seized. His joints are sometimes so stiff, that they can scarcely be bent, or, if they are, they remain in whatever situation they are placed. The pulse is often low and irregular. This disease may be owing to some violent affection of the mind disordering the brain and nerves, or to some acrid matter affecting them, either by its immediate contact, or by sympathy with the stomach, intestines, *uterus*, or some very sensible parts. To the same general causes are likewise to be ascribed the *emprosthotonus* and *opisthotonus*, and *tetanus*. And here we must rest; for to endeavour to explain more particularly, either how the passions, or an irritation of the brain or other sensible parts, bring on alternate convulsions or fixed spasms of the muscles, would be to no purpose, till we are better acquainted with the structure of these organs, and with that cause which immediately produces their contraction; points which will probably for ever elude our researches. All we know is, that whatever irritates or disagreeably affects the brain, nerves, or any of the more sensible parts, occasions continued spasms or convulsive motions, either in the parts themselves, if muscular, or in those with which they have any considerable sympathy; and that, when the nervous system is delicate, or the irritation great, almost all the muscles will be sometimes agitated with alternate contractions, or affected with a *tetanus* or general rigidity.

V. WIND in the stomach and bowels.

ALL our aliments, especially those of the vegetable kind, abound with air. In the time of digestion, part of this air is separated, and produces that flatulence or wind in the stomach and bowels with which many people are greatly troubled. But altho' flatulence arises from our aliments, some of which produce it more than others, yet strong and healthful people are seldom troubled with wind, unless

* Patholog. lib. 5. cap. 2.

less they either over-load their stomach, or swallow liquors that are in a fermenting state, and consequently full of elastic air. While, therefore, the matter of flatulence proceeds from our aliments, the cause which makes air separate from them in such quantity as to occasion uneasy complaints in the *primæ viæ*, is, almost always, a fault there; for, when, on the account of a weakness of the stomach and bowels, or an unnatural state of their nerves, the digestion does not go on properly, not only more flatulence is produced, but less of it returns again to a fixed state. Further, when, thro' the weakness of the coats of the stomach and intestines, the pressure upon its contents is considerably diminished, the air emitted by the aliments in digestion will not only be in greater quantity, but will expand itself more than in people of stronger organs. Agreeably to this it is observed, that dogs are much more troubled with wind and *borborygmi*, after tying the eighth pair of nerves, which sends many branches to the alimentary canal.

AIR in the stomach, being often hindered from rising by a slight spasm of the *cardia* or lower part of the gullet, either occasions an inflation of that organ, with other uneasy symptoms, or passes into the intestines; where, joined to more that is generated there, it distends them in some places, and consequently occasions a contraction in others. Hence pain; and, when the spasm gives way, the air, rushing thro' a narrow passage of some of the bowels, makes a rumbling noise: But, when the spasm in any part of the intestines, especially the *colon*, is greater, or lasts longer than usual, the air is more and more rarefied by the heat of the body; whence, their coats being over-stretched, great pain is occasioned, which is often attended with a vomiting. This is what is commonly called a flatulent or hysteric cholic. In some cases, certain parts of the alimentary canal are affected with such a fixed spasmodic contraction, that scarce any air passes either upward or downward; and more being daily generated, the stomach and bowels become at length greatly distended, or a tympany is produced. In this disease I have several times observed the swelling of the belly fall greatly, and the disease go almost quite off, while in the mean time very little wind
was

was discharged. This shews, when the alimentary canal returns to a sound state, that not only less air is generated from the food, but what has been produced may be mostly destroyed, or reduced to a more fixed condition.

VI. A great craving for food.

THIS may be owing to some humour in the cavity of the stomach stimulating its nerves, or to those nerves being so changed, that they are almost always affected with that sensation we call hunger, unless when food is newly taken into the stomach.

DOCTOR LOWER has observed, that hypochondriac and hysteric people are often troubled with an uncommon hunger, or *fames canina*; and while this lasts, they are almost quite free from other complaints, but that their usual ailments return with their natural appetite. In other cases, however, the morbid matter affecting the nerves of the stomach in hypochondriac and hysteric patients sometimes occasions a want of appetite and a *nausea*. In like manner, the true gout, when turned upon the stomach, according to the different sensibility of the nerves of that organ, or its being more or less fixed upon these nerves, produces very different effects; such as, an oppression, a languor, flatulence, want of appetite, and a sense of coldness in the stomach, or a violent pain with cramps and vomiting.

THE *malacia* and *pica*, common to women with child, and to girls affected with the *chlorosis*, proceed either from an acid or some other acrid humour in the stomach, or from its nerves being so changed by the state of pregnancy, as to produce a longing for certain foods, and other substances, which in these cases are generally most grateful to the taste, as well as apt to remedy the disorder of the stomach.

VII. A black vomiting.

ALTHO' not a few of the moderns, following the opinion of the antient physicians, have supposed the hypochondriac disease to be owing to an atrabiliary humour produced in the stomach, liver, or

spleen ; yet, in many hypochondriac patients, there is no such humour ; and where it is observed, it is only a symptom or consequence of that disease, but not its original cause.

PATIENTS who have been long afflicted with violent pains and cramps, or other disorders in their stomach, often throw up some dark coloured stuff, which is commonly nothing but blood that has lost its colour ; for altho', when blood is poured into the cavity of the stomach in a large quantity, it is soon vomited either in its fluid state, or coagulated ; yet, when it oozes slowly from the smaller vessels, it loses its red colour by lying long, and when thrown up, resembles the grounds of coffee. This kind of black vomiting is generally owing to one or more of the following causes, *viz.*

1. VIOLENT pain or cramps in the stomach ; the first of which may greatly increase the motion of the fluids in the small vessels, and the last may squeeze the globules of blood thro' the orifices of the small arteries, designed for conveying the gastric lymph only into the cavity of the stomach.

2. SCIRRHOUS tumours in the stomach beginning to ulcerate, or a rupture of some of the small vessels leading to them*.

3. A

* Further, scirrhus tumours in the stomach, by obstructing, in a great measure, the course of the blood through the indurated part, may occasion a more copious influx of this fluid into the neighbouring vessels ; whence the orifices of some of the exhaling arteries in the villous coat of the stomach may be so dilated, as to allow globules of red blood to escape with the thinner humours. This supposition is rendered, at least, not improbable by the following instance of bloody urine, occasioned by the womb pressing upon the neighbouring parts in time of pregnancy.

A gentlewoman, aged 19, began, in the fourth month of her first pregnancy, to make bloody urine, which continued till within fifteen days of the time of her delivery. This constant discharge, though weakening, yet was not attended with a quick pulse, nor any pain in the back or belly. After being delivered, she recovered and enjoyed perfect health, until about three or four months after she had conceived again, when the bloody urine returned and continued, as formerly, till a fortnight before she was brought to bed. In her third pregnancy, she was affected in the same manner, only she was much troubled with costiveness, which increased the other disorder ; and after this delivery, before she became again with child, she had sometimes returns of the bloody urine. During her fourth pregnancy, which happened in the 25th year of her age, the same symptom returned, but her loss of blood was now greater and more constant than ever ; so that she complained of great weakness, of a giddi-
ness.

3. A suppression of the *menfes* or hæmorrhoids, whence the blood that used to be evacuated by the *uterus* or *rectum* is turned upon the stomach, and partly discharged by some of its exhaling arteries.

I have known some hysteric patients affected with severe pains and spasms in their bowels, who did not vomit any black stuff, but often passed it by stool. In this case the black purging was owing to red blood making its way, in small quantity, into the cavity of the intestines; for that this atrabiliary humour, as it has been called, did not come from the liver, I was convinced by observing, that such patients as passed it by stool frequently vomited up, at the same time, bile of a natural colour. However, as hypochondriac and hysteric patients sometimes throw up a dark green bilious humour, there may be perhaps a few cases, in which a blackish liquor coming from the liver or gall-bladder may pass from the *duodenum* into the stomach, and be afterwards discharged by vomiting.

VIII. A sudden and great flux of pale urine.

THIS is reckoned by Sydenham the pathognomic sign of the hypochondriac and hysteric disease *. It has been ascribed by Hoffman to a spasm of the *sphincter* of the bladder †; and by Dr Cheyne to an obstruction of the perspiration ‡; but without sufficient reason.

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ness and loss of appetite; her pulse was quick and small, and, for above a month together, she had quotidian feverish paroxysms, generally of ten or twelve hours continuance. About the middle of the ninth month, her water returned to the natural colour; but, after lying in and recovering slowly, in six or seven weeks she grew as ill as ever. Her blood was now become so thin, that when she happened to cut her finger, it would scarcely tinge linen.

This disease, to which this person seems to have been predisposed by a laxity of the vessels of the kidneys, was most probably owing to the pressure of the womb on the iliac arteries, by which means the blood was thrown with greater force upon the emulgents; for, if the cause of this hæmorrhage had been merely the suppression of the *menfes*, it ought to have appeared much sooner than the fourth month. But however that may be, the patient found great benefit from the tincture of roses, the bark with elixir of vitriol, a strengthening plaster, and a diet consisting chiefly of mucilaginous substances, gellics, and the lighter flesh meats, with a little claret.

* *Epist.* ad D. Cole.

† *System. med.* tom. 4. part 3. cap. 6. § 16.

‡ *English Malady*, part 2. chap. 9.

THE real proximate cause of this symptom is always the same, viz. an increased motion, together with some degree of constriction of the secretory vessels of the kidneys; the first augments the quantity, and the second occasions the pale colour of the water: Altho' it must be owned, that this colour is principally owing to the quickness of the secretion of the urine, and of its passage thro' the bladder, before the finer parts are absorbed, and it has had time to acquire the common smell and taste, as well as colour, of that fluid.

THE causes of such an increased motion of the secretory vessels of the kidneys may be reduced to the following:

1. SUDDEN or violent affections of the mind. Thus people of a delicate frame and very moveable nerves will, soon after a fright, anger, or vexation, make great quantities of pale water. The whole nervous system being in such cases violently agitated, the secretory vessels of the kidneys are thrown into stronger and quicker alternate contractions than usual, and so make a larger secretion of urine. Add to this, that, as the perspiration is generally checked by disorders of the mind, the watery parts of the blood will be turned more upon the kidneys.

2. AN increased motion of the renal vessels is often owing to sympathy. Thus, as pain in the kidneys produces a *nausea* and vomiting, so a disagreeable sensation in the stomach and bowels from wind, crudities, or other causes, frequently affects the vessels of the kidneys with such an increased alternate motion as greatly to quicken the secretion of urine. Further, when the body is thrown into a general disorder, which happens in hysteric fits, the nerves of the kidneys are affected as well as others; and if the renal vessels are weaker, or more irritable than those of the other glands, the fluids, which are put into violent motion, will run off this way in the form of pale water. If the intestines are particularly weak, the person will be seized with a purging.

IN children breeding teeth, the irritation of the nerves of the gums sometimes affects the kidneys by consent, so as to occasion a considerable discharge of urine of this kind.

3. SINCE Sydenham has observed, that the hysteric disease does
often

often seize the kidneys, and occasion a pain, like that of a nephritic paroxysm *, may not that noxious matter in the blood, which is often the cause of nervous disorders, be thrown sometimes in such a manner on those parts as, tho' not to produce pain, yet so to stimulate their secretory vessels as greatly to increase the quantity of the urine? Nor is this more surprising, than that the same matter affecting the nerves of the stomach should produce, at different times, very different complaints. I have met with several instances of a great and long-continued flux of urine from an arthritic humour turned on the kidneys.

FURTHER, since a stoppage of urine, with a pain at the neck of the bladder, does in such patients sometimes proceed from the morbid matter producing a spasm there; may not a diminished secretion of urine, without any pain in the bladder or *urethra*, be owing in some cases to a spasmodic contraction of the ureters or secretory vessels of the kidneys?

I have had patients who, after a long fever, or some other tedious and weakening distemper, made a great quantity of pale water in the night, but in the day-time no more, sometimes less, than usual, and of a natural colour. This increased secretion did not generally begin at any certain hour in the evening, but soon after going to bed; and in the morning, after getting up, it gradually abated. Nay, a gentleman who had been in use, for ten or twelve days, to make from four to six English pints of pale water in the night, finding himself greatly weakened thereby, resolved to try what getting out of bed would do; and accordingly, at two in the morning, after having that night passed about a pint and a half of urine at twice, he rose and sat up for two hours, and then was able to make but about half a gill. After this he went to bed again, and, in two hours more, made near three quarters of a pint of pale water. This experiment was repeated some nights after with the same event.

THOSE who are troubled with this flux of limpid urine in the night, find themselves stronger, cooler, and in the best spirits in the evening.

* Epist. ad D. Cole.

evening, at which time their pulse is slowest; but, soon after going to bed, their pulse becomes quicker, they grow warmer, and begin to make water in great quantity; they are not refreshed with sleep, and, in the morning, they feel thirsty and languid, and have a quicker pulse than at other times.

THIS excess of pale urine, tho' most incident to people of weak nerves, yet, to distinguish it from the true hysteric *profluvium*, may be called hectic or colliquative, as coming in the place of those night-sweats which often exhaust such as have had their vessels weakened and their blood impoverished by some disease. When the vessels of the kidneys are relaxed, and yield too easily, or are too irritable, the fever raised in the night by the heat of the bed will naturally throw off the humour by them, rather than by the pores of the skin: But, in the day-time, the whole body, and the loins in particular, being kept cooler, the blood will be less rarefied, and will move with less force; whence the dilatation or increased alternate motion of the renal vessels will abate, and consequently the urine will be separated in much less quantity.

AN increased secretion of the *saliva*, is, like the copious limpid urine, owing to an unusual motion of the vessels of the salivary glands: And it may be observed, that, in patients whose salivary vessels are weakest and most irritable, a salivation will oftener happen, while, in those whose kidneys are most apt to be affected by any disorder in the body, a flux of pale water will be more frequent.

IX. A nervous atrophy.

A *marasmus*, or sensible wasting of the body, not attended with sweatings, any considerable increase of the excretions by urine or stool, a quick pulse or feverish heat, may deserve the name of nervous: Not that I would be thought by this to insinuate, that such a disease proceeds from a diminished secretion of the animal spirits, or from their vitiated quality. The fluid of the nerves does not seem to be that nutritious juice, by means of which the daily waste of the body is repaired*; and we know too little of its properties, to make

* See above, ch. 1. No. 9.

make it the foundation of our reasoning on the nature or cure of diseases. But this kind of atrophy, tho' not, perhaps, owing to any fault in the spirits, or even in the brain or nervous system in general, may yet deserve the name of nervous, as it seems frequently to proceed from an unnatural or morbid state of the nerves of the stomach and intestines.

THE influence of the stomach in the animal œconomy, is greater than is perhaps generally imagined: It not only contributes to the digestion of the aliment, but the whole system is either invigorated, or affected with a languor, according to the different disposition of its nerves. By proper food the nerves of the stomach are gratefully stimulated, and the whole body is thence enlivened and strengthened; so that, besides its use for nutrition, food in the stomach becomes, on account of its *stimulus*, altogether necessary, in some delicate nervous people, for keeping up the strength of the body and the due exercise of all its functions: And hence it is, that such persons become often faintish as soon as the greatest part of the food has passed into the intestines; that strong broths, tho' they may afford as much or more nourishment than some kinds of solid meat, yet do not satisfy the stomach, at least for any considerable time, or enable us to endure much labour; and that, according to the different disposition of the nerves of the stomach, different aliments are most grateful to it, and most invigorating to the body.

WE know, that an unnatural state of the nerves of the stomach may either produce a craving or an aversion to food; that low spirits and melancholy often proceed from that cause; nor is it to be doubted, that when the nerves of the stomach are, from certain causes, affected in a manner somewhat different, an indifference for food, a weak digestion, a languor, and coldness, a slow pulse and wasting, may be the consequences*.

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* Long continued grief, and other passions, too great application of mind, a gouty humour, or the morbid matter of some disease imperfectly cured, remaining in the body, and thrown upon the stomach, as well as other causes, may bring on such a state of the nerves of that organ, especially in those whose nervous system and alimentary canal are naturally

THE morbid affection of the nerves of the stomach, by sympathy, impairs the vigour and energy of the whole system; whence the motion of the heart and circulation of the blood will become slower and more languid, the body will be deprived of its natural heat, and be affected with a general weakness. The patient decays daily, tho' exhausted by no excessive evacuations, because his food is not converted into good chyle; and the nutritious fluid in the blood either does not possess its usual properties, or, on account of the languid manner in which all the operations of the body go on, is not applied to the several parts in such a way as to repair the waste they daily suffer. Further, the watching or want of refreshing rest, and low spirits or melancholy, which generally accompany this disease, may contribute to prevent the proper nutrition of the body.

THIS atrophy is generally attended with great costiveness, and sometimes with belchings, and other signs of flatulence in the alimentary canal. In some cases, the urine does not exceed the natural quantity; there is no thirst, and the tongue is clean: In others, the discharge by urine comes at length to be considerably increased, a drought prevails, and the patient decays faster.

THE pulse often differs little from what it is in health, except that it beats with less force. In some, it has a small degree of quickness; and in others, it becomes a good deal slower than is natural.

AFTER a nervous atrophy has continued long, and reduced the patient much, obstructions sometimes begin to be formed in the lungs, either from the languid circulation, or the fault of the fluids; a dry cough comes on, the pulse grows quick, and a hectic fever is kindled, which, together with the original disease in the stomach, increases the decay, and hastens the patient's fate.

SOMETIMES this disease, after it has brought the patient very low,

too delicate and sensible. I had some time since a patient subject to fits of the gout at the distance of several years, who, after labouring under such an atrophy as I am now treating of for eight or ten months, and using various medicines with no great benefit, was cured by a return of the gout to one of his great toes.

low, takes a sudden turn, without any apparent cause. The patient, who had little inclination to eat, has an uncommon craving and quick digestion, even of solid food, which used to lie remarkably heavy on his stomach: His pulse becomes quicker than natural, and his skin warm; his veins, which were contracted, appear swelled with blood; from being low spirited, he becomes more chearful, and daily grows stronger and plumper*: All which effects seem to proceed, in a great measure, if not solely, from some change in the nerves of the stomach and bowels.

IN other cases, this disorder goes off as slowly as it came on, and the patient does not recover fully till after a long time.

X. A nervous or spasmodic *asthma*.

EVERY difficulty of breathing, which is owing to a spasm of the bronchial vessels, or vesicles of the lungs, may, in a large sense, be called a nervous or spasmodic *asthma*; but as in most asthmatic ailments, where the lungs are obstructed, or loaded with phlegm, there is generally more or less of a spasmodic contraction excited in the air-vessels of the lungs; I should chuse to define a spasmodic *asthma* to be that species of difficult breathing, which is not owing to any obstruction in the lungs, or load of humours compressing their vessels, but to an uncommon contraction of their bronchial tubes and vesicles, whereby they do not yield, as usual, the pressure of the air in inspiration.

THE predisposing cause of this disease, is a particular weakness and delicacy or sensibility of the pulmonary vessels and nerves, which renders the musculo-tendineous membrane, connecting the annular cartilages of the *bronchia*, liable to be affected with a spasmodic contraction from such occasional causes as in a sound state of the lungs would not produce this effect.

THESE causes may be reduced to the following.

- I. ANY acrid or noxious matter in the blood, whether of the

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arthritic,

* See a case of this kind, in a young lad of 14 years of age, described under No. 13, of this chapter.

arthritic, rheumatic, or some other kind, thrown on the lungs, and irritating their nerves.

As a proof of this, I have known a person, affected with a fit of the spasmodic *asthma*, suddenly relieved by a gouty pain in his great toe, and become worse after a day or two, when this pain went off.

2. SYMPATHY with the stomach. When the nerves of this organ are disagreeably affected by wind, phlegm, or crudities, the lungs, if they are more than usually irritable, often suffer by sympathy, and are seized with a spasm. Further, when the stomach is much distended by wind, it may, by pressing upon the diaphragm, increase an asthmatic fit.

3. As hysteric fits and spasmodic cholics are often occasioned by violent affections of the mind; so I have known sudden fear bring on an asthmatic paroxysm, in a woman who was subject to frequent attacks of this disease.

4. A sudden diminution of the perspiration or contraction of the cutaneous vessels from cold, may, by turning the humours in too great quantity upon the lungs, occasion a fit. The cold affecting the cutaneous nerves may also, by sympathy, produce some kind of spasm in the air-vessels of the lungs.

5. Too great loss of blood will, in those who have very delicate or irritable lungs, be sometimes apt to produce asthmatic fits, rather than other nervous symptoms*.

6. SOME fixed obstruction in the lungs, which, at all times, makes the breathing somewhat less free than in perfect health, especially if any considerable exercise is used, and which, when some of the above-mentioned causes concur, produces an asthmatic fit, which indeed, strictly speaking, is of the mixed kind. And here it may be proper to observe, that among the many patients liable to periodical fits of the asthma, there are but very few who have not some obstruction or other obstacle constantly remaining in their lungs; so that a true nervous or spasmodic asthma, without any other fault in the lungs than an uncommon delicacy or irritability

* See an instance of this in Dr Barry's treatise on the three digestions and discharges of the human body, p. 294.

irritability of their nerves, is a disease which we seldom meet with; and, on this account, I have subjoined the following case.

A girl, healthful, well made, and of a seemingly good constitution, began, at the age of seven years, to complain of a pain at the lower part of the *sternum*. This pain, which returned after no certain intervals, became gradually more severe during the space of near two years; after which, in place of it, the patient began to be affected, at times, with a difficulty of breathing, which returned frequently, without observing any certain periods; as a week, a fortnight, or a month, would sometimes intervene between the fits. She was generally seized with the fits all at once; and after breathing with the utmost difficulty for half an hour, sometimes more than an hour, she would, of a sudden, become perfectly well, and fall a dancing immediately after with her companions. It was observable, that this girl had no complaint of her stomach, no cough, nor other apparent fault in her lungs; nor did she usually expectorate phlegm when the fit went off; and, except in time of the asthmatic paroxysm, breathed with the same ease as any person in perfect health. After having been subject to returns of this spasmodic *asthma* for above two years, she died of a continued fever, in which her head was greatly affected.

Was this distemper owing to some morbid matter in the blood, which first affected the parts about the *sternum*, or perhaps the *mediastinum*, with a painful sensation, and afterwards falling on the lungs, and irritating their nerves, occasioned a spasm or true cramp of their aerial vessels? I shall only add, that fits of the spasmodic *asthma* are sometimes preceded by a great discharge of pale urine; so that the patients can foretell them a day or two before they come on.

XI. A nervous cough.

A cough may be called nervous, when it does not proceed from any phlegm, obstruction, or other irritating cause in the lungs themselves, but from sympathy with some other part whose nerves are disagreeably affected. Of this kind is that dry cough which is oc-

caſioned by worms, or by teething in children. A cough with very unuſual ſymptoms has alſo been owing to water in the *pericardium* and other diſorders of the heart, when the lungs themſelves appeared to be found. But inſtead of making any further obſervations on this ſubject, I ſhall give a particular account of a very extraordinary cough of the truly nervous or ſympathetic kind.

A girl aged eight, in January 1760, was ſeized with a dry cough, which continued for two or three months, notwithſtanding ſeveral remedies that were uſed to remove it. In October following the cough returned with as much violence as before, and with this difference only, that it was rather more ſevere when ſhe ſat up than when ſhe lay in bed. Altho' her ſkin was cool, her pulse ſcarce quicker than uſual, ſome blood was taken away, and a vomit was given, but without any good effect. Upon a ſuſpicion that this cough might be owing to worms in the ſtomach or inteſtines, ſhe took ſome powder of tin, and two doſes of rhubarb with calomel. The cough ceaſed in eight or ten days after uſing theſe medicines, altho' no worms were brought away by them.

TOWARDS the end of December 1760, this girl, after having been in good health for fix or ſeven weeks, was again ſeized with a dry cough, for which ſhe was twice blooded without any advantage; but ſhe found ſome relief, for a few days, by a bliſter applied to her back.

ABOUT the middle of January, the cough became more conſtant and ſevere when ſhe ſat up, but never affected her when ſhe lay in bed. On the third of February, when I was called, I found the following ſymptoms.

WHILE ſhe lay in bed, ſhe had no cough, no difficulty in breathing, nor any pain or uneaſineſs in her breaſt; her ſkin was cool, her tongue moiſt and clean, her appetite good; and ſhe was as chearful as uſual. Her pulse beat then about 90 times in a minute; it was of a moderate ſtrength, but a little irregular. When ſhe ſat up in bed, her pulse became quicker by ten or twelve ſtrokes in a minute, but ſhe ſtill was free from the cough and every other complaint; and in this poſture ſhe continued moſt part of the day.

When

When she stood either on the bed, or on the floor, or when she sat on the bed-side, or on a chair, she was immediately seized with the cough, which continued without intermission, until she lay down again. The cough was dry and convulsive, for she could not restrain it for one moment; it was attended with a pain in the *sternum*, about an inch or more above the xiphoid cartilage, which pain she never felt in any degree when she lay down or sat up in bed.

WHEN she stood on her feet, her pulse became very small and irregular, and beat at the rate of 200 times in a minute.

AT different times, in the months of February and March, I frequently repeated the following experiments with a view to discover more of the nature and cause of this uncommon cough.

1. WHEN she lay on her back, across the bed, with her legs hanging over it, she was free from the cough, but was immediately troubled with it when she sat up.

2. WHEN she sat up in the bed, or sat on the floor, with her thighs and legs in a horizontal posture, she did not cough at all.

3. WHEN she sat in the bed, and drew up both her legs as close as she could to her thighs, she was then attacked with the cough, and with the pain in her breast.

4. WHEN she sat in her bed on the bolster and pillow, with her thighs and legs inclining a little downwards, she had no cough.

5. WHEN she kneeled down, either on the floor or in the bed, with her body erect, she was immediately seized with the cough, and the pain in her breast.

6. WHEN she lay on her back, with her head and shoulders as low, or a little lower than her body, she coughed without intermission, as she did also in an erect posture.

7. IN a prone posture, with her head as low, or lower than her body, she coughed incessantly, and was like to be suffocated; but as soon as her face was a little raised and supported on the bolster or pillow, the cough ceased.

THE cough, the pain in her breast, and sense of suffocation were greater, and her pulse was smaller, quicker, and more irregular,

gular, when her head was low, than when she stood upright; but lying low on her face seemed to give her still greater uneasiness than lying low on her back.

8. SHE lay on either side with ease, and without coughing, unless when her head was as low, or lower than her body.

9. WHEN she sat or stood with her feet in warm water, she had neither any difficulty in breathing, nor inclination to cough, nor pain in her breast; but she coughed without intermission, the moment her feet were taken out of the water.

WHEN she sat with her feet in the warm water, her pulse beat 120 times in a minute; and, when standing in it, between 130 and 140 times. When she stepped out of the water, and stood on the floor, the cough instantly returned, and her pulse rose to 200 in a minute.

10. WHEN the heat of the water was reduced (by pouring some cold water slowly into it) from above 100 degrees of Fahrenheit's scale, to about 70, the cough returned with its usual violence; and altho' she sat in a chair, her pulse rose from about 120 to near 190, and became small and irregular. After this, upon gradually adding boiling water, so as to raise the heat of the bath to 88 or 90 degrees, the cough stopt, and her pulse became fuller, much slower, and more regular.

11. WHEN she coughed the most violently, if the soles of her feet were only made to touch the warm water, she grew immediately easy, and continued so, altho' her feet were not wholly immersed.

12. WHEN one of her feet was taken out of the bath, the cough was not prevented, by increasing the quantity of warm water, so as to make it not only cover the other foot, but also a good part of the leg.

13. AFTER her feet had been, for some minutes, in water heated to about 114 degrees, one of them was taken out of it, and that instant the cough returned with its usual violence; notwithstanding that foot and ankle continued, for some time, to be warmer

er than the body naturally is, or than was necessary to prevent coughing upon putting the foot into the warm water.

14. WHEN one of her legs was taken out of the water (warmed to about 96 degrees) and wrapt in a dry or wet piece of flannel, whose heat was at least 114 degrees, she coughed as usual; but was relieved as soon as her foot was again put into the warm water.

15. WHEN her feet were covered with dry sand, heated to above 110 degrees, she coughed with the same violence she used to do on the floor. Nor was the cough, either when she sat or stood, prevented by the flannel wrung out of hot water, and applied round her feet and legs; altho' an equal or a greater degree of heat was by this means communicated to these parts than by the *pediluvium*.

16. WHEN her hands were dipt in warm water, she continued as free from the cough as when her feet were bathed. But a bottle filled with hot water and held between her hands had no such effect.

17. WHEN one of her feet was taken out of the bath, altho' the hand of that or the other side was put into water of an equal or a greater heat, she coughed without intermission; but as soon as both hands were dipt in the warm water, she coughed no more.

18. I made her breathe over the steam of hot water, when one of her feet was taken out of the *pediluvium*; but this did not prevent the cough.

19. WHEN she lay with her head as low or lower than her body, (N^o 6. and 7.), warm water then applied to her hands or feet had no effect in preventing or lessening the cough; but in every other posture it kept her quite easy.

20. IF one or both hands were dipt in cold water, she was presently seized with the cough, and with the pain in her breast, whether she lay in bed or sat with her feet in warm water. The same thing happened when her palms were applied to a quart bottle of cold water; with this difference, that the cold water instantly raised her cough; whereas the cold bottle took two or three seconds before it could produce that effect. The cough was also raised by applying a bottle full of cold water to her stomach.

21. WHEN

21. WHEN she lay with her legs hanging over the bed-side, (N^o 1.), she began to cough as soon as the soles of her feet touched some cold water.

22. THE putting her hands in cold water, when she lay in bed, not only excited the cough, but raised her pulse from about 90 to above 180 strokes in a minute.

THESE experiments were often repeated between the 3d of February and the 8th of March; but some time after this, I found the following difference with respect to some of the above mentioned symptoms.

23. ON the 1st and 4th of April, when she lay across the bed with her head supported by a pillow, and her legs hanging over, (N^o 1.), she was immediately attacked with the cough, and her pulse became so small and quick, that I could not exactly count it; but I was sure it did not beat less than 18 or 20 times in five seconds (N^o 9.). Upon raising her legs, so as to bring them to a horizontal posture, the cough immediately ceased, and her pulse in a minute after beat only ten times in five seconds. As soon as her legs were allowed to hang down again, the cough returned with its usual violence.

24. ON the 5th April, by putting one of her hands into cold water when a-bed, she was seized with the cough, and her pulse became very small, and beat at least 20 times in five seconds (N^o 22.).

OF late, she felt more uneasiness and pain in her breast, with a greater sense of suffocation, when she was seized with the cough, either upon a change of posture, or putting her hands into cold water. And her pulse, which used to be about 90 when she lay in bed, was now at 96 in a minute: but her skin continued cool, she had no thirst, and her appetite was good.

IT will be proper to add, that she has had no expectoration from the beginning.

FROM the above facts it appears,

(a) THAT an erect posture does not excite the cough, unless either the legs or thighs be much bent, or in a depending or perpendicular situation. See N^o 1. 2. 3. 4. and 5. above.

(b) THAT

(b) THAT a depending situation of the legs did not, at first, occasion the cough, unless when the body was erect; but afterwards, that posture of the legs had this effect, altho' the body lay horizontal. N^o 1. and 23. Her pulse also became a great deal quicker in this attitude than it had formerly been, either when she sat up, or when she stood; whence it would seem that the cause of the disease had been gradually increasing from the third of February to the 1st of April. N^o 9. 10. 23. and 24.

(c) THAT when the head and shoulders are as low, or lower than the body, the cough is still more severe than when she stood upright. N^o 7.

(d) FROM the experiments already related, I was ready to imagine that the cough might be owing to some tumour or other fixed cause in the breast, which, in certain postures, so strongly irritated that part of the lungs which it touched, as to occasion a constant convulsive motion of the muscles of respiration; but the following experiment, which I frequently repeated, soon dissipated this theoretical illusion.

WHEN my patient lay in bed, upon extending one of her feet, so as to bring it nearly to a right line with the leg, she coughed violently, and her pulse rose from 94 in a minute to 118 in five seconds: But when her hands were either strongly bent inwards, or extended outwards, or when she pulled strongly or raised a considerable weight with them, no coughing ensued.

WHEN the cough was raised by stretching her feet, warm water applied to her hands immediately put a stop to it.

FROM this experiment, as well as N^o 23. it may appear, that this extraordinary cough did not depend on any fixt obstruction or tumour within the *thorax* irritating the lungs in certain postures. But, in this patient, the nerves of the lungs seem to have been endued with an uncommon degree of sensibility, and to have had a peculiar sympathy with the legs and feet; whence, as often as they were in a depending situation, or the nerves, tendons, and ligaments at the ancles, were stretched, an uneasy sensation was felt in the lungs, which occasioned an incessant cough. Altho' the sym-

pathy between the lungs and the other parts, appears to have been less remarkable, yet the shock which their nerves suffered from cold water (No. 20. and 21.) was so strongly felt in the lungs, as to occasion a pain in the breast, together with the cough.

WHEN the head and shoulders were lower than the body, the cough was more severe than in any other situation, probably, because in that posture the respiration is less free, and the blood passes with more difficulty thro' the lungs.

(e) WARM water did not, by its pressure on the nerves or blood-vessels of the feet, prevent the cough, because it was excited by cold water, whose weight is greater. Neither did the *pediluvium* produce this effect by its heat alone, or even by its heat and moisture; for sand or wet flannel of an equal or greater degree of heat applied to the feet, did not prevent the cough. No. 15. and 16.

(f) As the effects of the *pediluvium* cannot be deduced from its rarefying the blood by its heat, neither can they be owing to any derivation of this fluid towards the inferior extremities; because warm water, whether it was applied to the hands or the feet, had the same influence in stopping the cough; and as soon as the soles of her feet touched the water, the cough ceased. No. 16. and 11.

(g) IT remains, therefore, that warm water, by its particular action on the extremities of the nerves to which it is applied, renders the whole system less sensible of any irritation; whence the too delicate lungs are less affected in consequence of their sympathy with the inferior extremities (d). However, when the patient lay with her head lower than her body, the warm water did not then prevent the cough; because, in that position, the irritation in the lungs was too great to be wholly removed by the anodyne power of the warm water: And, for the same reason, it seems to have been, that the *pediluvium* did not prevent the pain within her breast and the cough, which were raised by dipping her hands in cold water. No. 6. 7. 19. and 20.

(h) IT appears from the above experiments, that warm water affects our nerves very differently, not only from a dry heat, but also from warm steams, or cloths dipt in hot water; a fact which seems

seems not to have been known, or at least not sufficiently attended to, and which perhaps may afford some useful hints in practice. No. 14. and 15.

(i) SINCE warm water, applied to the nerves, has a superior anodyne affect, not only to substances that are warm and dry, but even to warm steams or vapour; it is easy to see, how clysters of warm water may give relief in pains of the bowels and other abdominal *viscera*, altho' they do not communicate more heat to the great guts than they possessed before.

(k) LASTLY, the effects of the warm water in this case appear the more remarkable, as a pill consisting of half a grain of *opium*, and three grains of *asa fetida*, given every evening and morning, for several days, had not the least effect in either preventing or lessening the cough.

BETWEEN the 20th of January and the 25th of March, a variety of remedies were prescribed for this patient, without any advantage, *viz.* vomits, blisters, and an issue between the shoulders, the bark, powder of tin, rhubarb with calomel, pills of *opium* with *asa fetida*, boluses of theriaca with camphire and valerian.

TOWARDS the end of March, I put her on a course of pills made of the extract of hemlock, which she continued for two months. About the middle of May she began to have less pain in her breast, and less sense of suffocation and coughing, when she sat up out of bed, or walked through the room. Upon the 22d of May, these complaints left her altogether; and on the 28th of that month, the cough was neither raised by standing nor walking, nor when her head was laid lower than her body: Also cold water applied to her hands had now no effect in exciting the cough or pain in her breast. On the 30th of May, after walking a little abroad, the cough returned for a day or two. Upon the 3d of June, after having made a journey of about ten English miles in a chaise, the cough attacked her with as great violence as ever. Being now fully convinced, that this ailment was not owing to any fixt obstruction in the lungs, but to an uncommon delicacy or sensibility in their nerves, I ordered for her pills of extract of gentian and *lima-*

tura martis, which she took twice a-day for about ten weeks. Towards the end of July, the violence of the cough began to abate, and, for the first eight or ten days of August, she was seldom troubled with it. On the 10th of August, it returned and continued to the 2d of September, when it left her entirely. In the month of November following, she had a slight attack of the cough and uneasiness in her breast; which symptoms returned, for one day, in September 1762, since which she has been very rarely affected with them in any considerable degree. It was observed, that the returns of her cough after September 1761, were always owing to her using exercise too freely.

XII. PALPITATIONS of the heart.

1. IN those whose nervous system is easily moved, any sudden and strong passion, but especially fear, will produce palpitations, and an irregular motion of the heart, by rendering it more irritable, and, at the same time, by forcing upon it the venous blood in greater quantity than usual *.

2. THE regular motion of the heart may be also disturbed by its sympathy with the stomach, when this organ is disordered, by wind, noxious humours, worms, or other causes; by the suppression of some habitual evacuation; by some acrid matter in the blood falling on the heart itself †; by inflammations or obstructions in it or the *pericardium*, and by *polypi* or ossified valves; for these causes either render the heart more irritable than in a natural state, or disturb the free motion of the blood through the great vessels adjoining to it.

XIII. THE pulse often varying in quickness, strength, and fullness,

* Fear or surprize, seems to occasion a sudden contraction of the right *sinus venosus*, and, perhaps, also of the adjoining trunks of the *venæ cavæ*; for I frequently feel, upon any surprize, a sudden contraction about my heart, while the veins in my hands and fingers feel as if they were distended with blood.

† I have often seen palpitations, which, as far as I could judge, were owing solely to an arthritic humour affecting the heart.

ness, not only in different patients, but in the same at different times.

To account for these variations of the pulse, it will be sufficient to mention, briefly, the general causes of a strong and weak, hard and soft, quick and slow pulse.

1. As a strong pulse is owing to the ventricles of the heart expelling, with a considerable force, that quantity of blood which they can contain; so a weak pulse may proceed from a debility of the ventricles, whence a proper impulse is not given to that fluid; or it may be owing to a too great irritability whereby the ventricles contract before they are sufficiently filled; or to the want of a free circulation of the blood through the lungs, whence it returns in too small a stream to the heart.

2. A hard pulse is owing either to a too great density of the blood, or to an obstruction, or, oftener, a spasmodic contraction of the vascular system, particularly the capillary arteries; in which case the blood passing with difficulty into the veins, the arteries must feel tense and hard *.

THIS pulse often occurs in pleurifies, and other inflammatory diseases. It is to be observed, however, that in inflammations of such parts as are very sensible, and have a remarkable sympathy with the heart, while the pain produces a kind of spasmodic contraction of the arterial system, it often renders the heart so irritable, that, tho' the pulse feels somewhat hard, yet it is very small; because the ventricles contract before they are sufficiently filled with the returning blood: And this is frequently the case in inflammations of the stomach, bowels, and *uterus* †. On the other hand, when the lungs or liver are inflamed, the pulse is generally softer and fuller, because these parts have but little painful feeling; and therefore the vascular system is seldom affected with any spasm. It

* I have known some people, whose pulse, in a natural state, was harder than that of most others in the greatest inflammatory diseases. It is not probable, that in such the coats of the arterial system were more tense, and the passage from the arteries into the veins straiter than usual.

† See above, p. 241.

It is, however, to be observed, that an inflammation of the external membrane of the liver or lungs is attended with considerable pain, and a hard pulse, as in a pleurisy.

3. A too soft pulse is owing either to a laxity of the whole vessels, and particularly of the capillary arteries, or to a thinness or watery state of the blood, which passes into the veins and secretory vessels so easily, that it can exert little of its force in dilating the arteries.

A soft pulse is more common than a hard one, in those patients who are subject to nervous or hysteric complaints; because too thin blood and a laxity of the vascular system are more common in such, than dense blood and a too great tension or spasmodic contraction of the arteries, which occasion a hard pulse.

4. A pulse quicker than natural must be owing to one or more of the following causes, *viz.* an increase of the stimulating quality of the blood, its quicker return to the heart, or a greater degree of sensibility, and consequently a greater aptitude for motion in the heart.

(a) THE stimulating quality of the blood is increased, by its becoming too dense or fizy, by external heat, by fresh chyle, such especially as is prepared from animal food, or acrid and heating aliments; and by the mixture of any noxious humours bred in the body, or of malignant or poisonous *effluvia* received from the air.

(b) THE blood is made to return in greater quantity to the heart by all kinds of exercise, sudden fear, and other strong passions.

(c) THE sensibility, and consequently the irritability of the heart * is increased by various affections of the mind, or whatever increases the general sensibility of the nervous system, by sympathy with the other parts, especially the stomach and intestines, when these are pained, or affected with a disagreeable sensation, by an arthritic, scorbutic, or some other morbid humour thrown upon the heart; and by obstructions and inflammations in any part of
the

* See above, p. 296. &c. p. 324. &c. and where it is proved, from undoubted experiments and observations, that the irritability of the muscles of animals depends on their sensibility.

the body, but especially in the lungs, *pericardium*, or in the heart itself.

5. A pulse slower than natural must be owing either to a diminution of the stimulating quality of the blood, its slower return to the heart, or a less degree of sensibility, or aptitude for motion in that organ.

(a) THE stimulating quality of the blood is lessened by external cold, by too weak or too spare diet; and by the blood being not of a proper density, but poor and watery from a weakness of the vascular system. Hence, after great evacuations, the pulse not only often becomes low, but very slow. I have seen, in patients recovering from fevers, or in women ten or twelve days after child-bearing, the pulse fall under fifty strokes in a minute, and rise afterwards to about seventy, its natural standard when the patients were stronger and their vessels fuller. In such cases, besides the poorness of the blood, and the want of a sufficient quantity of it, a general languor and debility of the whole body probably concurred to make the pulse so very slow.

(b) THE return of the blood to the heart becomes slower when the body is at rest, especially in a horizontal posture, and when the mind is not disturbed by passions.

(c) THE sensibility and irritability of the heart are lessened by age, deep sleep, and every medicine or distemper that impairs the general sensibility of the brain and nervous system, as *opium*, a lethargy, *coma*, apoplexy, &c. Further, as the heart is often rendered more irritable by its consent with the stomach and bowels, when these parts are disagreeably affected by wind, the arthritic matter, or other causes; so its irritability seems, in some cases to be lessened by its sympathy with these parts, when their nerves are affected in a different manner *. Thus, worms or viscid phlegm in the stomach.

* "Venæ ——— plerumque satis sano corpore, si stomachus infirmus est, subeunt et quiescunt." Celsus de medicina, lib. 3. cap. 6.

stomach and bowels, or a violent pain of the spasmodic kind affecting them, will sometimes make the pulse much slower than natural, as well as irregular: And long continued grief, melancholy, or low spirits, by impairing the vigour of the whole nervous as well as vascular system, may render the pulse slower than in its natural state, unless some morbid cause quickens the motion of the heart.

FROM what has been said of the causes of the quickness, slowness, strength, and fulness of the pulse, it will easily appear, why, in nervous, hypochondriac, and hysteric disorders, the pulse is often so different, not only in various persons, but in the same person at different times. I shall therefore only add a few instances of the effect of those ailments in making the pulse quicker or slower than usual.

(1.) A lady aged 38, who had lost a great deal of blood in child-bed, on the eighteenth day after her delivery, at six in the morning, was seized with a sharp pain above the *os pubis*, darting towards the *anus*. This pain sometimes extended upwards, and then over to the right side in the direction of the *colon*. Notwithstanding her having taken twenty-five drops of *laudanum*, she complained of a *nausea* and inclination to vomit about half an hour past seven, and before two in the afternoon, she vomited six or seven times. About eleven in the forenoon, having had a clyster administered with *asa fetida*, she had two stools, and passed a great deal of wind. Her pulse, which, when she was taken ill, beat 60 times in a minute, about seven in the morning began to grow quicker, and before two in the afternoon rose to 130; but became feebler and smaller in proportion to its quickness. At this time, as scarce any thing would stay on her stomach, a broth-clyster was injected, with 40 drops of *laudanum* in it: After which she lay quiet for two hours, and her pulse came down to 120. From four in the afternoon to ten, she took every hour some panada, with a little claret and cinnamon, by which her pulse was reduced to 100 in a minute, and began to be fuller. After this, as the complaints in her stomach and bowels decreased, her pulse returned to its natural strength and slowness.

A quick pulse, as in the above case, is carefully to be distinguished from a quick pulse occasioned by an inflammation, or a common fever. In the former it is soft, and neither full, hard, nor contracted; it becomes smaller as it increases in quickness; nor is it commonly attended with any great heat or thirst; but the surest mark is, that it becomes slower upon eating a little flesh-meat, drinking a glass of claret, or using castor and *opium*; all which are hurtful when the pulse is quickened by inflammation, and for the most part in fevers till their decline.

HOWEVER, it may be proper to observe, that a quick pulse, occasioned by pain from spasms or wind in the stomach or bowels, may, especially in such as are plethoric, upon continuing long, change its nature, and from being merely nervous or spasmodic, become at last inflammatory; that is the consequence of an inflammation produced in the part affected with pain.

(2.) AN unmarried lady, between 30 and 40 years of age, was seized with a severe pain in her lower belly, and had been ill of it near two days before I was called. I found her pulse at the rate of 70 strokes in a minute, and of a natural softness. I ordered her, at bed-time, 25 drops of *laudanum* with as many grains of *rhubarb*. She was easy through the night; but next morning, when the effects of the *laudanum* were over, and the *rhubarb* had begun to operate, her pains returned with greater violence, and she had two stools. About noon, the pains increased, and then her pulse, which, in the morning, had been just as the day before, became smaller and slower, so that at two in the afternoon it did not beat above 56 in a minute. At that time she complained of a lowness, and a coldness through her whole body. I directed her to take some panada with wine and nutmeg, and ordered a clyster with fifty drops of *laudanum* in it. This soon removed the pain, and restored the pulse to its natural fulness and quickness; the coldness went off, and her skin grew rather warmer than usual.

IN these two cases we see, from the same general cause, *viz.* a sharp pain in the bowels, opposite effects, a quick pulse in the first, and a slow one in the second; and by the same medicine and diet,

viz. laudanum, panada, and wine, we find the pulse made slower and fuller in the one, and quicker and fuller in the other. What might be the reason of such a difference is hard to say: Was it owing to the different kind of *stimuli* affecting the nerves of the bowels, or rather to the different constitution of these two patients?

AN acute pain in any part generally brings on an inflammation, and quickens the pulse; but in people subject to nervous or hysteric complaints, a violent pain in one side of the head, in the stomach, or intestines, often renders the pulse slower and more languid.

WHEN pain produces inflammation, it not only excites the vessels of the part into stronger and more frequent alternate contractions than usual; but the heart and whole arterial system are, by sympathy, rendered more irritable. On the contrary, when an irritation or pain in any part occasions a spasm, or continued contraction of its vessels, no inflammation is produced in it; and the heart and vascular system being, by sympathy, also commonly affected with some degree of spasm, perform their alternate motions with less freedom and readiness; whence the pulse becomes slow, small, and sometimes irregular, and the whole body feels cold.

DOES then the difference between pain, with or without inflammation, consist in the vessels of the part affected being agitated, in the former case, with an uncommon alternate contraction, and, in the latter, with a continued spasm?

WHEN, in delicate people, we meet with pain producing a quick but soft and feeble pulse, and without any considerable increase of the heat of the body, we may suppose, either that, altho' the vessels of the pained part be affected with a spasm, yet the heart does not suffer in this way, but is only rendered more irritable by the pain; or that, notwithstanding those vessels may be agitated with a greater alternate motion than usual, yet, on account of the weak state of the blood or laxity of the solids, scarce any degree of inflammation is produced.

(3.) A gentleman betwixt 30 and 40 years of age, who, for several years, had been much troubled with flatulent complaints, was, after

after an error in diet, seized with a pain about the middle of the *abdomen*, striking into his back, which soon became so intolerable, that, after having vomited up several doses of *laudanum*, and had clysters injected to no purpose, he was obliged to have recourse to the *semicupium* for relief. His pulse, which, in a natural state, beat about 64 times in a minute, was, by the violence of the pain, reduced to 44 strokes in that time, and was besides small, feeble, and often irregular. The warm bath not only relieved the pain in the bowels almost instantaneously, but also rendered his pulse full, soft, and regular, tho' somewhat quicker than it used to be when he was in health. Some time after he came out of the warm bath, the pains returned with considerable violence, and his pulse also became slow, small, and irregular; but upon having recourse to it again, he was immediately made easy, and the pulse returned nearly to its natural state.

(4.) A youth of fifteen, of a strong make, and seemingly healthy constitution, had, for some time, been subject, once in six or eight weeks, to a violent pain in his belly, with an apprehension of immediate danger. During the time he was most troubled with these cholic pains, his pulse commonly beat only fifty times in a minute; but as soon as, by the use of laxatives, and aromatic bit-
ters, he had got free of this complaint, it returned to its natural quickness, which was about 80 strokes in that time.

(5.) ANOTHER lad of 14 years of age, of a thin and delicate habit, and of quick and lively feelings, whose pulse in health used to beat between 70 and 80 times in a minute, about the beginning of June 1757, was observed to be low-spirited and thoughtful, to lose his appetite, and have a bad digestion. Altho' he lost flesh daily, yet he had no night-sweats, no extraordinary discharge of urine, and was costive. His tongue was clean, his skin cooler than natural, and when in bed, his pulse beat only 43 times in a minute; nay, about the middle of July, when reduced almost to skin and bone, his pulse, in a horizontal posture, did not exceed 39. About the end of August, his distemper took a sudden turn; he then began to have such a craving for food, with a quick digestion, that

he grew faint unless he eat almost every two hours; he had two or three stools a-day; his pulse beat from 96 to 110; his skin was warm, and his veins, which scarce could be seen before, became now turgid with blood. The strong apprehensions he formerly had of dying left him, he was sure he should recover; and accordingly, by the middle of October, he was plumper than ever he had been before. Towards the end of November, his appetite became moderate, and his pulse gradually returned to its natural state.

It was observable, that the pulse was slowest towards the evening, and generally of a proper strength and fulness.

SINCE, with all my attention, I neither could discover the cause of this patient's first complaints, nor of the sudden and contrary turn which they took afterwards, I shall not pretend to reason on his case; but I thought it deserved to be mentioned as a good instance of a nervous atrophy, and of the effect of such disorders in making the pulse much slower than ever it has been observed in a natural state.

XIV. PERIODICAL headaches.

THESE either affect almost the whole head, especially in the fore-part, or only one side of it; sometimes no more than one of the eyes, with part of the fore-head and temple of the same side. They generally return once a day, nearly at the same hour, and as regularly as the fit of a quotidian ague. In some cases they are attended with a visible swelling, not only of the eye affected, but also of that side of the forehead. Sometimes the eye seems to sink within its orbit; at other times, nothing can be observed but that the eyes want their usual lustre, and look as if the person had watched long, or drunk too much.

THE most common causes of periodic headaches in those who are subject to nervous disorders, are,

I. SYMPATHY with the stomach, by which the nerves chiefly of the fore-part of the head suffer; and the small vessels to which they are distributed are either affected with a continued spasm, or agitated with uncommon alternate contractions and relaxations; in consequence

quence of which the patient feels a pain, straitness, fulness, and pulsation about the fore-head and temples.

2. A viscid or acrid humour obstructing or irritating the small vessels of the *pericranium*, muscles of the head, or *dura mater*, and consequently affecting the nerves of those parts with a painful sensation. This may be often no other than a rheumatic, gouty, or scorbutic humour falling chiefly on the head.

3. A particular weakness, delicacy, and sensibility of the nerves of those parts of the head; whence, from sudden changes of weather, errors in diet, fatigue of body, strong passions, intense application of mind, suppression of ordinary evacuations, or even from slighter causes, these nerves being easily susceptible of pain, the small vessels to which they are distributed become affected either with violent alternate contractions and relaxations, or with a fixed spasm. This seems to be confirmed by observing, that women liable to these periodic headaches suffer most severely about the menstrual periods; at which time it is well known, that issues and other sores become generally more painful and inflamed, as being more irritable and easily affected than the other parts. In any general indisposition, those parts which are least firm and sound suffer most.

How these headaches should return every day, or sometimes once in two days, is a hard question. We know, that intermitting fevers observe very regular periods: And I have seen epileptic patients have fits once or twice every day, or once in two days, almost precisely at the same hour. Hysterical convulsions and other diseases have also been observed sometimes to be regularly periodical.

Does the morbid matter in such cases, after being dislodged by the violence of the paroxysm, require a certain time before it is again collected or deposited on the parts affected in such a quantity as is sufficient to produce a new fit? Such is the obscurity of Nature in many of her operations, that we meet almost every where with appearances of which we are unable to give any satisfactory explanation. However, both in natural philosophy and medicine, it is often sufficient, at least for the purposes of life, to know the certainty of some particular *phenomena*, altho' we cannot account

count for them: *Sufficit, si quid fiat intelligamus, etiamsi quomodo quidque fiat ignoremus.* Cic.

XV. A giddiness.

THIS may proceed from some of the causes which have been mentioned above, as producing periodic headaches, especially when they affect the anterior part of the brain or *dura mater*.

MANY people of a delicate, nervous, and vascular system, after stooping and suddenly raising their head, are apt to be seized with a *vertigo*, which is sometimes accompanied with a faintness. In this case, the vessels of the brain, being too weak, seem to yield more than usual to the weight of the blood, when the head is inclined; and afterwards, when it is suddenly raised, and the blood at once descends towards the heart, those vessels do not contract fast enough, so as to accommodate themselves to the quantity of blood remaining in them: At the same time the brain, on account of its too great sensibility, is more affected than usual by any sudden change in the motion of the fluids thro' its vessels.

It seems to be owing to an uncommon delicacy and sensibility of the *retina*, and indeed of the whole nervous system, that some people become so giddy as to be in hazard of falling, if they look steadfastly into a glass that is kept constantly moving before them, or at any object that is turned swiftly round.

XVI. A dimness of sight without any visible fault in the eyes.

THIS sometimes proceeds from the stomach*; in which case, the patients are only affected with it at particular times, when that organ is out of order, and by sympathy affects the *retina*, optic nerves, or that part of the brain from which they take their rise. I know a lady much troubled with a sourness in her stomach, who, when this increases to a greater degree than usual, sees every thing indistinctly, as if a thick smog or mist was before her eyes; nor does she get quite free of this, till by chalk, or crabs eyes, lime-water, *magnesia*

* See Lommii Observat. Med. lib. 2.

nesia alba, vomits, and bitters, she has destroyed, in a great measure, the acidity in her stomach.

I had some years since a patient of a very delicate nervous system, whose eyes, when his stomach was much troubled with acidity and flatulence, were sometimes rendered so very sensible, that looking stedfastly on a crimson colour, or coming suddenly from a bright light into a dark room, or from this last into the sunshine, would occasion a giddiness and pain above his eyes, together with a dimness of sight, and a bilious vomiting.

XVII. Low spirits, melancholy, and a *mania*.

1. IN cases of an irregular gout, when the arthritic matter falls upon the stomach and bowels, it frequently produces a *nausea*, flatulence, low spirits, and other uneasy symptoms. In such, wind pent up in the stomach or intestines occasions a disagreeable tho' not painful sensation, attended with a faintness, languor, and depression of mind. But at other times, when this arthritic matter has left these parts, we may observe, that a greater degree of flatulence, occasioned by errors in diet, will have no such effect. Low spirits, therefore, in hypochondriac and hysteric cases, may be frequently owing to some morbid matter in the blood, flatulent and improper aliments, or other causes affecting the stomach and bowels with a particular sensation; which, tho' not painful, nevertheless is attended with great dejection of mind.

2. Low spirits may be occasioned by obstructions in the hypochondriac *viscera*, viz. the stomach, liver, &c. But as obstructions often happen in those parts without any remarkable dejection of mind, whenever they are attended with this symptom, it must be owing principally to the nature of the obstructing matter, or rather to a particular morbid state of the nerves of those *viscera*.

3. A *mania*, and the higher degrees of melancholy, may proceed from some noxious matter in the blood, carried, from the *viscera* of the lower belly, or other parts where it was chiefly lodged, to the brain. Of this I shall give an instance or two, that some time ago occurred in my practice.

(a) A

(a) A gentlewoman upwards of 30 years of age, who had been long troubled with wind in her stomach and bowels, indigestion, faintness, languor, palpitations, and sudden fits of terror, with a pulse generally quick, but variable, having been for some little time much freer from these complaints than usual, on the 24th of August, became all at once deprived of her reason. During the nights, and in the mornings, she talked incoherently; but throughout the day she had some intervals of reason. While she continued in this way, her pulse was better than usual, and she was quite free from her ordinary nervous symptoms. She had no sharp pain in her head, but complained of an uneasy sensation and great confusion in it. Being costive, she took some aloetic pills; but could not be prevailed on to use any other medicine. However, in a few days, she grew much better; and by the 5th of September entirely recovered the use of her reason, but relapsed in some degree into her old complaints of flatulence, indigestion, and palpitation.

(b) A gentleman aged between sixty and seventy, after having been for some years free from the gout, began to have constant complaints of his stomach and bowels, and at last was seized, all at once, with a *delirium*, which, by the application of sinapisms to the soles of his feet, went off in a few hours. In two days the *delirium* returned, when, by blistering his legs, a pain came into one of his great toes, upon which he recovered his senses entirely. In this manner the gouty humour moved backwards and forwards between his head and feet, for near two months, till at last, being more fixed in the brain, it brought on a continued and violent madness, which no remedy could lessen. In this state he obstinately refused almost every kind of food, and died in a few weeks.

4. SUDDEN terror, excessive grief, or other violent passions of the mind, in people whose nervous system is very delicate, may affect the brain so as to produce a continued *mania* or melancholy. But in what manner the passions, or the morbid matter of nervous diseases, change the state of the brain, or *common sensorium*, and occasion such disorders, is entirely unknown.

XVIII. THE *incubus*, or night-mare.

IN this disease the patient, in time of sleep, imagines he feels an uncommon oppression or weight about his breast and stomach, which he can by no effort shake off; but groans, and sometimes cries out, tho' oftener he attempts to speak in vain. He imagines himself to be struggling with strong men or devils, to be in a house on fire, or in danger of being drowned in the sea or some river. In attempting to run away from danger, or climb up a hill, he fancies he falls back as much after every step as he had advanced before. The terror excited by the frightful ideas attending the night-mare sometimes occasions a tingling of the ears, and a tremor over the whole body.

THIS disorder has been commonly supposed to proceed from a stagnation of the blood in the *sinuses* of the brain, or in the vessels of the lungs; or from too great a quantity of blood being sent to the head.

THE horizontal posture in time of sleep, and the pressure of the stomach upon the *aorta*, in a supine situation, have been thought sufficient to occasion a more than usual distension of the *sinuses* and other vessels of the brain; and the weight of the heart pressing on the left auricle and large trunks of the pulmonary veins may, it is said, prevent the easy return of the blood from the lungs, and so produce an oppression and sense of weight and suffocation in the breast *. But not to enter into a particular examination of these opinions, which are far from being satisfactory, I shall only observe, that, if they were true, some degree of the night-mare ought to happen to every person that lies on his back, especially after eating a full meal. Further, if a horizontal situation could overcharge the brain with blood, so as to occasion the *incubus*, how comes it that people, who remain for some time in an inverted posture, do not feel this disease beginning to attack them? And why does a slighter degree of the night-mare sometimes seize people who sleep in an erect situation in a chair †? As the weight of the stomach,

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* See Dr Bond's essay on the *incubus*, chapter 2.

† Something of this kind I have experienced myself, not only after eating, but also before supper, when my stomach was out of order, and troubled with wind.

even when filled with food, can have scarce any effect upon the motion of the blood in the *aorta*; so the pressure of the heart is by much too small to be able sensibly to retard the motion of that fluid in the pulmonary veins; otherwise, people exhausted by tedious diseases, who generally lie on their back, would be constantly affected with the *incubus*.

WE know, that certain medicines or poisons, worms, and even corrupted bile, or other humours, by disagreeably affecting the nerves of the stomach, produce an oppression about the *præcordia*, wild imaginations, frightful dreams, raving, and insensibility: And there is no doubt, that low spirits, melancholy, and disturbed sleep, often proceed from a disordered state of the stomach. Is it not probable that the night-mare has its seat chiefly in the same organ? If epileptic fits often proceed from the stomach, why may not the *incubus*, which has been considered by Galen as a nocturnal or flighter epilepsy, have its seat in that part? People troubled with nervous and hypochondriac ailments, and who have delicate or flatulent stomachs, are more subject than others to this disorder. A heavy or flatulent supper greatly increases the night-mare in those who are predisposed to it. The sympathy of the stomach with the head, heart, lungs, and diaphragm, is so remarkable, that there can be no difficulty in supposing the several symptoms of the *incubus* to arise from a disagreeable affection of the nerves of that organ.

WHEN my stomach has been out of order and troubled with wind, I have often perceived a flighter *incubus* seize me before I was fully asleep, the uneasiness of which would make me get up suddenly. As soon as I was quite awake, I was generally sensible I had been affected with a weight and uneasiness about my stomach, attended with a faintness, and some sort of oppression or suffocation about my breast, as if the circulation in my lungs had been a good deal obstructed. While I sat up in bed, or lay awake, I felt nothing of these symptoms, except perhaps some degree of uneasiness about my stomach; but, when I was just about to fall asleep, they began to return again. In this way I have often gone on
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for two hours or more in the beginning of the night. At last, I found that a dram of brandy, after the first attack, kept me easy the whole night. This remedy has never failed to succeed with me, the few times I have had occasion to try it; for of late, since my stomach has been pretty sound, I have seldom felt in my sleep any of those uneasy sensations which resemble the night-mare.

FROM what has been said, it seems probable, that in the *incubus* the stomach is commonly the part primarily affected: I say commonly, because symptoms like those of the night mare may sometimes arise without any fault in the stomach. Thus, I have known asthmatic patients, whose lungs were much obstructed, who, in time of sleep, were greatly oppressed with a sense of suffocation, and disturbed with uneasy dreams: And Dr Lower mentions a patient, who, tho' he could sleep pretty easily with his head inclined forward, yet, in the opposite situation, he was always soon awaked with horrid dreams and tremors; the cause of which appeared, after his death, to have been a great quantity of water in the ventricles of the brain.

THE *incubus* is most apt to seize persons when lying on their back; because, in this position, on account of the stomach and other abdominal *viscera* pressing more upon the diaphragm, we cannot inspire with the same ease, as when we sit up, or lie on one side. Further, in that situation of the body, the food seems to lie heavier on the stomach, and wind in it does not escape so readily by the *æsophagus* or *pylorus* as in an erect posture, when these passages are higher than the other parts of the stomach *. We are only affected with the night-mare in time of sleep, because the strange ideas excited in the mind, in consequence of the disordered state of the stomach,

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* When I have been liable to be attacked with a sensation of faintness at my stomach, I have found it always worse when I lay on my back in the night-time, and become better when I got out of bed, or sat up in it. And a middle aged woman who, in the morning was frequently subject to faintings, found that she could prevent them by getting up as soon as she perceived them about to come on. Further, when the miliary eruption does not come out properly in women after child-bearing, they are often affected with a sense of faintness, and with an oppression in their breathing, which symptoms are commonly worse when they lie on their back, than when they sit up in bed.

are not then corrected by the external senses, as they are when we are awake *; nor do we, by an increased respiration or other motions of the body, endeavour to shake off any beginning uneasy sensation about the stomach or breast. The *incubus* generally seizes one in his first sleep, but seldom towards the morning; because at this time the stomach is much less loaded with food than in the beginning of the night.

If the night-mare were owing to a stagnation of the blood in the lungs from the weight of the heart, or in the *sinuses* and other vessels of the brain from the horizontal posture of the body, it would become greater the longer it continued, and would scarce ever go off spontaneously: But we know, that this disease, after affecting people for some time, often gradually ceases, and is succeeded by refreshing sleep; for as soon as the load of meat or wind, or other cause disagreeably affecting the nerves of the stomach, is removed, the oppression and weight on the breast, wild imaginations, frightful dreams, &c. vanish, as all these proceed originally from the disorder of the stomach. It is worth while, however, to observe, that as neither wind, tough phlegm, nor crudities in the stomach, do ever occasion the symptoms of the hypochondriac disease, unless the nerves of that organ be indisposed; so neither a horizontal posture, sleep, nor heavy suppers, do ever produce the night-mare, at least in any considerable degree, unless the person be predisposed to it from the particular condition of the nerves of his stomach: And here I shall just remark, that a *plethora*, as well as other causes, may so affect the nerves of the stomach as to give rise to the *incubus*. Hence, a suppression of the *menfes* in women sometimes occasions this, as well as other disorders of that organ. It must, however, be acknowledged, that a *plethora*, by rendering the circulation thro' the lungs less free, may help to produce, or at least increase, the oppression of the breast in the night-

* I had, some years since, a patient affected with an *eryspelas* in his face, who, when awake, was free from any confusion in his ideas; but no sooner did he shut his eyes, altho' not asleep, than his imagination began to be greatly disturbed: He thought himself carried swiftly through the air to distant regions; and sometimes imagined his head, arms, and legs, to be separated from his body, and to fly off different ways.

night-mare: And hence it is perhaps that young men, who abound in blood, are often troubled with this disorder.

IT has been observed above *, that violent or long continued complaints of the nervous, hypochondriac, or hysteric kind, sometimes terminate in an apoplexy, palsy, jaundice, dropfy, tympany, or *phthisis*. Now, from what has been said, it will not appear strange, that the brain and nerves may, by the continuance or frequent repetition of such shocks, be so weakened or disordered, that not only fatuity, a deep melancholy, or *mania*, but also a palsy or an apoplexy may ensue. Further, as nervous disorders are often owing to some morbid matter in the blood, when this leaves the stomach and intestines, or other parts where it used chiefly to fix, and is thrown in a great measure on the brain or origin of the nerves, it is easy to conceive how a palsy or apoplexy may be the consequence.

AGAIN, since hypochondriac and hysteric disorders are sometimes occasioned by obstructions in the abdominal *viscera*, and often give rise to them; and as from a bad digestion the chyle must be ill prepared, it will appear why those diseases do sometimes terminate in the jaundice or dropfy.

IT has been observed also, that patients much afflicted with those ailments have at length fallen into a *tympanites*, which may be thus accounted for. I have shown above, that the great predisposing cause of nervous, hypochondriac, and hysteric disorders, is a particular weakness and delicacy or uncommon sensibility of the stomach and bowels; whence, from slight causes, they are often affected with spasms. Now, when the spasmodic contractions of the alimentary canal do not continue long, the wind that was pent up is allowed to move from one place to another, and is at last expelled either upwards or downwards: But when the stomach and intestines, by reason of their weakness, and small but continued spasms, have been inflated by slow degrees, the irritation occasioned by this distension increases the spasm so much, that the air continually generated by the aliment in time of digestion is mostly retained,

* See above, p. 531.

tained, or at least is not discharged in such a quantity as to relieve the patient, or sensibly to diminish the swelling of the belly.

LASTLY, a *phthisis pulmonalis* may also be the consequence of nervous disorders, when the morbid matter producing them falls chiefly upon the lungs, or when the vitiated chyle or blood forms obstructions in that organ.

AND here it may be worth observing, that while the morbid matter producing the hypochondriac disease chiefly affects the stomach and bowels, the patients are always apprehensive, and often greatly alarmed from any trifling increase or variation of their complaints, as if they were in immediate danger of dying; but after this matter has left its old seat, and, by fixing on the lungs, has brought on an incurable *phthisis*, they generally cease to be apprehensive or fearful, and cherish the hopes of life to the last. The reason is, that when the lungs are affected, there are no such uneasy feelings excited in the body, nor fear and dispondency in the mind, as when the stomach and intestines suffer, which are not only possessed of a much more delicate sensibility than the lungs, but have also a more remarkable sympathy with the brain and whole nervous system.

CHAP.

C H A P. VII.

OF the CURE of NERVOUS, HYPOCHONDRIAC, and HYSTERIC
DISORDERS.

ALTHOUGH it may be said in general, that these disorders are more troublesome and lasting than dangerous, yet, as they proceed from various causes, the danger, as well as the cure, must be often very different. Thus, when they are owing to an original delicacy of the whole nerves, or a debility of those belonging to the stomach and intestines, they seldom prove quickly fatal, but scarce ever admit of a thorough cure. When they are occasioned by an arthritic matter in the blood, their cure will be almost as difficult as that of a chronic rheumatism, or of the gout itself; and in such a case, perhaps the best that can happen is, that the morbid matter may throw itself off, by regular fits, in the extremities. When they arise from too great or too small a flux of the *menfes*, if the *uterus* can be restored to a sound state, the nervous symptoms will vanish of course. When great and confirmed scirrhus obstructions in the abdominal *viscera* are the causes of hypochondriac or hysteric complaints, they are not only incurable, but likely to prove soon fatal. When they proceed from worms, phlegm in the stomach and bowels, or violent affections of the mind, they may be often and sometimes speedily cured. Lastly, when intemperance in eating or drinking has brought on nervous ailments, they may be almost always lessened and sometimes cured by a proper diet, moderate exercise, and a few medicines.

BUT, however troublesome and obstinate nervous disorders often may be, they have some advantages attending them; for the weak
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state of the blood and vascular system in many of these cases renders such patients much less subject to inflammatory diseases than those of a stronger constitution.

FROM the account I have given of nervous, hypochondriac, or hysteric disorders, it will appear, as has been already observed, that their cure, far from being the same, must differ according to the various causes from which they proceed: and that the numerous warm, aromatic, stimulating, and foetid medicines, which have been called nervous, or antihysteric, however proper they may be in some cases, are nevertheless hurtful in others.

IN treating, therefore, of the cure of those diseases, I shall not attempt to lay down any general method, to answer in all cases or circumstances, even for the same symptoms; but shall endeavour to point out that particular treatment which seems best suited to the case, according to the various causes from which it may arise.

BUT, before I proceed, it will be proper to observe, that, as it is generally in the power of medicine to relieve, it is frequently beyond the power of art to eradicate the disorders we now treat of; and therefore it may be often of use to intimate this to our patients, especially to such as have fortitude enough to bear those evils which can neither be wholly prevented, nor fully cured. It is further necessary to acquaint every patient, that, without a long perseverance in a course of medicines, diet, and exercise, no great or lasting benefit can be expected. To this purpose is the following passage of Montanus, which equally deserves the attention of such patients as are affected with nervous ailments, and of the physicians who undertake their cure. “In curatione *hujus morbi* (*sciz. hypochondriaci*) non licet prae finire tempus mensis unius aut anni, sicut in aliis contingat; sed oportet in toto *vitæ suæ* tempore curationi operam dare, interdum curationi, interdum præservationi, attendendo.” *

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* Confil. 30.

THE general intentions in the cure of nervous disorders, may be reduced to the two following, *viz.*

I. To lessen or remove those predisposing causes in the body which render it peculiarly liable to nervous ailments.

II. To remove or correct the occasional causes which, especially in such as are predisposed, produce the numerous train of nervous, hypochondriac, and hysteric symptoms mentioned in the preceeding part of this work *.

I. THE great predisposing cause of nervous disorders is, as I have shown, a too great delicacy or uncommon sensibility of the nerves in general, or of those of the stomach and intestines, or other organs, in particular. If this fault in the constitution could be effectually cured, we should always have it in our power to lessen the violence of nervous symptoms from whatever cause they might arise, and to prevent most of those which proceed from sudden impressions made on the mind. But when the fault in the nervous system, alimentary canal, or other parts, is original, *i. e.* natural to the constitution, and not the consequence of some disease or irregularity in living, it does not admit of a perfect cure: The utmost that can be done is to lessen it.

THE best remedies to answer the first intention of cure, are either such as not only strengthen the stomach and bowels, but the whole body, or those which, by their peculiar action on the extremities of those nerves to which they are applied, lessen, for a time, the too great sensibility of the whole system.

I. THE remedies which have been found by experience to communicate greater strength to the body are,

(a) BITTERS. Of these, I most commonly use the *radix gentianæ*, *summitates centaurii minoris*, and *cortices aurantiorum*; the two former, as being less nauseous and heating, than many of the other bitters; and the last, partly on account of its agreeable flavour.

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* See above, p. 530. &c.

These bitters may be put into any of the stronger white wines; but if the patient be troubled with acidity in the first passages, they ought to be infused in brandy or boiling water. The watery infusion will be rendered more agreeable to many stomachs, by adding to each English pint of it three ounces of the *aqua cinnamomi fortis*, or *aqua aromatica* of our Dispensatory *.

THE strength, as well as the dose of these bitters, must be adapted to the constitution and circumstances of the patient. If they heat too much, they must be weakened, or taken along with some drops of the elixir of vitriol †. When bitters lie heavy on the stomach, and lessen, instead of mending, the appetite, they ought to be omitted, and the cure must be attempted by other remedies.

(b) The BARK. This is more strengthening and less heating than any of the bitters. It may be given either in substance or decoction, or infused in cold or in boiling water, in lime-water, wine, brandy, or rum.

THE bark in substance, frequently disagrees with delicate stomachs, and occasions sickness, gripes, and sometimes a looseness. An infusion or decoction of it in water, especially if some grateful aromatic, such as cinnamon or nutmeg, be added, is less apt to produce these effects; but when infused in brandy, with some bitters or aromatics, it will agree well with most people. The bark in substance often sits lighter on the stomach, if a glass of red port be taken after every dose of it; and the gripes and purging, which it occasions in some, may be certainly prevented by adding, for a few days, the *confectio Japonica* to it; for after the stomach and bowels have been accustomed to the use of the bark, it generally occasions either much less disturbance, or none at all.

FOR several years past, I have frequently joined the bark and bitters in the following form.

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* The officinal compositions mentioned in these observations are always understood to be those of the Edinburgh Dispensatory, unless the contrary is expressed.

† Mead *Monita medica*, p. 109.

R. Cort. Peruvian. Pulv. unc. iv.

Rad. Gentian.

Cort. Aurant. ana unc. i. fs. Mifce.

Infunde in spir. vin. Gall. lib. iv. in *balneo arenæ* per dies vi. et cola.

OF this tincture, I generally give one table-spoonful, with four or five spoonfuls of water, every morning, an hour and a half before breakfast, and between seven and eight in the evening. I sometimes add to each pound of this tincture, an ounce or more of the *sp. lavend. comp.* which improves its taste, and makes it fit better on some stomachs.

I have myself taken the above tincture in the morning, for eight months together, and with remarkable advantage. For three or four years before, I had been much troubled with wind in my stomach, a giddiness, and sometimes a faintness. I observed in the morning, soon after taking this medicine, a grateful sensation in my stomach, accompanied with better spirits than I had at any time through the day, or than I ever found from drinking wine, even when I used it freely. I have ordered this tincture to many patients, who have taken it for two or three months successively, and, after intermitting it for some time, have begun again. Most of them have found benefit, and those most who used it longest. The cases were chiefly weak and windy stomachs, with a general delicacy or debility of the nervous system *.

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WHEN

* A married lady aged 40, of a thin habit and delicate nerves, had been complaining for some years of a general weakness and feebleness through her whole body, especially in her limbs, with a pain sometimes in her stomach and belly, which she attributed to wind. I prescribed for her the tincture of the bark, &c. which she took once a-day for near two years, intermitting now and then a week or ten days. It had a most sensible effect in strengthening her, and never failed to raise her spirits. When, after intermitting this medicine for a longer time than usual, her old complaints have begun to return in a less degree, a few doses of it have, almost always, put her to rights again. Another married lady, aged between 30 and 40, of a delicate nervous system, and affected with wind in her stomach, giddiness, flying pains through her body, frequent fits of looseness in a morning, feebleness, and low spirits, was, by the use of the same tincture for near two years, (intermitting it now and then for a month or more at a time), cured, in a great measure, of all her complaints, except that she continued sometimes to be troubled with the pains, and something of the low spirits, though in a much less degree than formerly.

Were

WHEN acids do not disagree, twenty or thirty drops of the elixir of vitriol may sometimes be taken with advantage in each dose of the tincture. This elixir taken twice a-day, in this or a larger dose, in spring-water alone, has often good effects in strengthening the stomach and restoring a decayed appetite; and is generally an excellent cooler when the stomach-complaints are attended with any degree of febrile heat, thirst, and a white tongue.

ALTHO' the bark is preferable, as a strengthener, to any of the bitters, yet it does not wholly supersede their use. The bark alone will not fit so well on many stomachs, as when it is joined with an agreeable bitter; and I think I have found more benefit myself from the above tincture, than from the bark alone either in substance or decoction. With regard to the safety of taking, for a long time, the bark, against which many have had great prejudices; I can say, that I do not recollect its proving hurtful in any case in which I have ordered it, unless where it happened to disagree with the patient's stomach. About fourteen years since, I swallowed in sixteen days, near four ounces of it in substance, when I laboured under a catarrhus cough, without feeling any bad effects from its astringent quality. In a tertian intermittent, attended with a cough and spitting, after the use of vomits and some pectorals, I have prescribed the bark in the usual quantity, without the breast being any ways hurt by it. I have had repeated experience of its virtues in curing a hoarseness after the measles, unattended with a fever or difficult breathing; and in the chincough, when given early, and before any obstructions are formed in the lungs, I have found it one of the best remedies. Lastly, The success of the bark in resolving indolent glandular swellings*, may shew that it is not possessed of any considerable obstructing quality.

(c) STEEL. There are few medicines that so remarkably strengthen the stomach and bowels, and indeed the whole body, as iron and its preparations. The astringent quality of this metal was not unknown.

Were it necessary, I could add many other cases in which the same remedy has been remarkably useful.

* See Medical inquiries and observations, vol. 1.

known to Dioscorides, who recommends, for a weakness of the stomach and intestines, water in which a red hot iron has been extinguished.

THE *sal martis* was in great esteem with Riverius; but Sydenham preferred the filings of iron to all its preparations*.

THE filings have been commonly prescribed from five to fifteen or twenty grains; but altho' this last quantity will heat many people, yet, so different are constitutions, that some will bear a much greater dose; nay, I know a gentleman, who, for a weakness in his stomach and indigestion, has taken every day, for some months together, about 230 grains of the filings of iron, divided into three doses. It is obvious, however, that these filings will act variously as they are finer or coarser, and according to the quantity of an acid in the stomach and bowels. They sometimes occasion, especially in the more delicate constitutions, a disorder in the first passages; in which case Sydenham has advised a few drops of *laudanum* to be taken with them at bed time; but fifteen grains or a scruple of *theriaca* will have as good or a better effect.

THOSE who cannot take the *limatura martis* will often bear Mynsicht's tincture, the chalybeate wine, and pyrmont or other steel waters of a weaker nature. I know a lady, whom six or eight grains of the filings of iron will purge more strongly than an ordinary dose of rhubarb, and yet fifteen or twenty drops of the *tinctura martis Mynsichti* give her no disturbance.

I sometimes order this tincture, or the *mars saccharatus*, to be taken at the same time with the tincture of the bark and bitters above mentioned; but commonly I advise the chalybeates only at those times when the patients intermit the bitters.

THE chalybeate waters, altho' they contain but a very small proportion of iron, are often observed to have remarkable effects in strengthening the body. Particularly, the waters of Bath in Somersetshire have been of great use to many, who, from a weak state of the stomach and bowels, were affected with low spirits and other nervous complaints.

It

* Dissert. epistol. ad D. Cole.

IT may be worth while to observe, that, notwithstanding the remarkable effects of chalybeates in many diseases, yet these medicines, in a state of solution, or in a saline form, do not seem to enter the blood; for the late ingenious Dr Wright, having made a dog, who had fasted 36 hours, swallow a pound of bread and milk, with which he had mixed an ounce and a half of *sal martis* dissolved in a sufficient quantity of water, and filtrated; he opened the dog an hour after, and collected from the thoracic duct near half an ounce of chyle, which did not suffer the least change of colour by dropping into it a tincture of galls; altho' this same chyle, after $\frac{1}{4}$ of a grain of *sal martis* was dissolved in it, acquired a deep purple colour from that tincture *.

IF *sal martis* and other preparations of iron do not enter the blood, it is obvious, that they may produce their effects solely by strengthening the stomach and intestines; whence not only the digestion of the aliment will be better performed, but, by means of that remarkable sympathy which subsists between the alimentary canal and the whole system, a greater degree of vigour will be communicated to every part of the body: for there is nothing more certain, than that we feel ourselves either vigorous and healthful, or feeble and sickly, as the nerves of the stomach and bowels are in a sound or an infirm state.

THE above medicines (*a, b, c,*) are to be used not for days or weeks only, but often for many months together, otherwise no great or lasting benefit is to be expected from them. In some cases, it may be necessary not to omit their use, wholly, for years; for when the cause of any disease is deeply rooted in the constitution, those medicines which are proper for removing it must be taken almost like our diet, not only regularly, but for a very long time.

IN such cases, it may be best to take the bark and bitters chiefly in the winter and spring-season, intermitting their use now and then for a week or two; and in the summer to drink either some of

* Philosophical Transactions, for 1750, vol. 50. part. 2. p. 595.

of the chalybeate waters at the wells, or a gill or more of the Pyrmont or Hartfell Spa * thrice a day on an empty stomach.

(d) The COLD BATH. Nothing perhaps strengthens the nervous system more sensibly, or gives a greater spring to all the vessels, than cold bathing; for altho' the water only acts immediately on the cutaneous nerves and vessels, yet its strengthening power is, by sympathy, communicated to the inmost parts of the body. The cold bath, like the former remedies, ought to be long continued. The most proper seasons for it, are the spring, summer, and autumn. It is enough, especially for those of a spare habit, to go into the cold bath three or four times a week; but as it tends to make people thinner, those who are too plump may use it daily. When the stomach, liver, or other *viscera* are much obstructed, or otherwise very unsound, the cold bath is improper, since by turning the blood with more force than usual upon these parts, it may increase, instead of lessening the patient's complaints.

MANY instances might be given of the good effects of cold bathing in strengthening people of delicate constitutions, and making them less subject to nervous ailments; but as so much may be found to this purpose in Sir John Floyer's history of cold bathing, I shall only observe, that I have known it of great service to several women, who, chiefly from a weakness of their nervous system, were very liable to suffer abortion; and that a young lady, whose nerves seemed to have a very great degree of sensibility, from the intolerable pain which she felt from blisters, and from the very uneasy sensation which was occasioned by every red pimple that rose on her face, found more benefit from a long course, first of the cold bath, and afterwards of sea-bathing, than from bark, bitters, chalybeate waters, and various other remedies.

To

* The Hartfell Spa is a water which issues from a mountain of that name near Moffat in North Britain. It has a strong chalybeate together with an aluminous taste, is much saturated with iron, and seems also to contain an aluminous salt. It is destitute of that spirit observable in the Pyrmont water and those of Spa near Liege, but retains its virtues longer, and may be carried to a great distance without being sensibly weakened. It is an excellent strengthener, and has often been found serviceable in weaknesses of the stomach and intestines with indigestion and flatulence. For a more particular account of this water, see *Essays Physical and literary*, vol. 1. and *Philosoph. Transact.* vol. 50. part 1.

To prevent mistakes, it may be proper to mention here, that while I recommend bitters, the bark, elixir of vitriol, chalybeates, and cold bathing, as the best strengtheners of a delicate nervous system, I do not mean that all these are to be used, especially at once, by the same patient. In some cases, the tincture of the bark with some bitters will be sufficient. In others, more benefit may be found from steel in substance, or from the chalybeate waters; and sometimes cold bathing may succeed, or at least make the cure more compleat, after internal strengtheners have in a good measure failed. I shall only add, that when nervous complaints, arising principally from a delicacy of the nervous system, are attended with a quick pulse and a preternatural heat, bitters and steel are improper; but an infusion of the bark in cold water, with elixir of vitriol, will often prove useful.

(e) AIR. As a cool and dry air braces and imparts vigour to the whole body, so nothing tends more to relax and weaken than hot air, especially that which is rendered so by great fires, or by stoves in small rooms.

WHEN the stomach and bowels are weak, the body ought to be well guarded against cold, especially in winter, by wearing a thin flannel waistcoat next the skin; for this will keep up an equal perspiration, and defend the alimentary canal from many impressions to which it would otherwise be subject, upon every sudden change from warm to very cold weather.

(f) ALIMENT. The food ought to be nourishing, but of easy digestion, and suited to the stomach of the patient. Fat meats and heavy sauces are hurtful. All excess is to be avoided. Valetudinarians ought never to eat more at once than they can digest with ease. Every time the stomach is over-loaded, its strength is impaired, and its nerves are disordered; but when one eats moderately, not only the stomach, but the whole body is invigorated and repaired. Above all things, heavy suppers ought to be avoided, since the stomach is more apt to be oppressed, with the same quantity of food, in a horizontal than in an erect posture; and since the digestion

gestion goes on more slowly in time of sleep than when we are awake.

WINE in excess enfeebles the body, and impairs the faculties of the mind. A few glasses in time of eating, or after it, may be useful; but more will load a weak stomach, and retard digestion. The best time to drink a little wine, is upon an empty stomach; for the liquor being, in that case, less weakened and more readily applied to the nerves there, must have the greater effect in strengthening them. When my stomach has been weak, and when, after having been indisposed, I had hot palms, was languid, and apt to sweat upon motion, I have often found myself much better for a glass of claret, and a bit of bread, an hour or more before dinner; in this case, the wine cooled me, made my pulse slower, and gave me more spirits and strength. I have ordered claret in the same way to others, before dinner, and between seven and eight in the evening, with advantage. When children are weakly, have a tendency to the *scrophula*, or are inclined to the rickets; or when they have been much reduced by a fit of teething, I find a little claret once or twice a day, upon an empty stomach, an excellent strengthener, and the best *succedaneum* to the bark, which many children will not take.

THESE good effects of wine thus used, seem not to have been altogether unknown to Celsus, who tells us, "*Si quis vero stomacho laborat, non aquam, sed vinum calidum, bibere JEJUNUS debet* *."

WINE in general is preferable to malt-liquor, as being lighter, less apt to ferment, and less flatulent. For common drink, water alone, or with a little wine, is the lightest and best; but when the stomach and bowels are troubled with acidity, water mixed with a small proportion of rum or brandy is greatly preferable to wine or malt-liquor.

UNDER this head, it may not be improper to observe, that the frequency, now-a-days, of stomach-complaints, and nervous ailments, as they are commonly called, may be partly owing to

* De medicina, lib. I. cap. 8.

the too great use of tea. I once imagined tea to be in a great measure unjustly accused; and that it did not hurt the stomach more than an equal quantity of warm water; but experience has since taught me the contrary. Strong tea drunk in any considerable quantity, in a morning, especially if I eat little bread with it, generally makes me fainter before dinner than if I had taken no breakfast at all; at the same time it quickens my pulse, and often affects me with a kind of giddiness. These bad effects of tea are most remarkable when my stomach is out of order.

(g) EXERCISE. Exercise is of such use for strengthening the nervous system, that, without its assistance, the most powerful medicines will prove often ineffectual. Of all kinds, riding on horse-back has been justly esteemed the best: It has been particularly extolled by Sydenham in hypochondriac and hysteric disorders. It greatly promotes digestion, sanguification, the distribution and secretion of all the fluids; and strengthens the whole body as well as the stomach and bowels. Riding is preferable to walking, as it shakes the body more and fatigues it less. But it is proper to observe, that any great exercise, especially riding on horseback, after a full meal, will disorder the stomach, and retard digestion, instead of promoting it.

THE ingenious Dr Gilchrist of Dumfries has recommended sailing, as a kind of exercise well adapted to the cure of nervous complaints arising from a weak state of the blood and alimentary canal, and has given some instances of its good effects *. But as we find it very difficult to prevail with any patient in this place to undertake a long sea-voyage, I can say little on this head from my own experience. However, I have not only been well informed, that a gentleman who had been long subject to epileptic fits at land, was never seized with them when at sea; but a young gentleman, lately my patient, who had a very delicate nervous system, and whose stomach and intestines were so uncommonly sensible, that a single stool, procured even by the *elixir sacrum*, made him faintish; and vomiting or purging was almost sure to bring on fainting fits with
flight

* See his Treatise on the use of sea-voyages in medicine.

flight convulsions: This person, I say, had his constitution so changed while he was at sea, that altho' during a voyage of four or five weeks, he vomited much every day, and purged frequently; yet he had neither any faintings, nor was sensible of any particular weakness in his stomach or bowels. After this voyage, he had no return of those fits to which, for some time before, he had been liable, till at the distance of eight months, when he applied a blister to the under part of his breast; the pain of which, when the blister was taken off, occasioned faintings with slight convulsions.

FRICITION of the legs, arms, trunk of the body, and *abdomen*, with a flesh-brush, with flannel or a coarse linen cloth, is a kind of exercise that strengthens, promotes the circulation, and is particularly beneficial when the bowels are weak.

LYING too long in bed will weaken and relax; while early rising, like gentle exercise or cool air, will brace and invigorate the body.

(*b*) AMUSEMENT. The mind ought to be diverted and kept as easy and chearful as possible; since nothing hurts more the nervous system, and particularly the concoctive powers, than fear, grief, or anxiety.

2. BUT as the remedies (*a, b, c, d, e, f, g, h,*) above mentioned, however proper for mending a delicate state of the nerves in general, or of those of the alimentary canal in particular, must often be used a considerable time before they can produce any great effects, it becomes frequently necessary to have recourse to medicines of another nature, in order to palliate those uneasy symptoms with which nervous and hysterical people are often affected.

THE principal remedies of this kind are the following, *viz.*

(*a*) SUCH as weaken, during the time of their operation, the sensitive power of the nerves, and consequently lessen those pains, irregular motions, or spasms which arise from any usual irritation. The chief of these is *opium*, which, when applied in sufficient quantity, to the nerves of any sensible part, not only lessens their power of feeling, but by sympathy also that of the whole system: By this quality it often gives sudden relief in many violent disorders of the nervous and hysteric kind. It is of great use in fixed spasms, as

well as in alternate convulsions of the muscles, and in pains not attended with inflammation; in a weakness, lassitude, and yawning, occasioned by too great a flux of the *menfes*, in flatulent colics, and sometimes in the true spasmodic *asthma*, where there is no obstruction of the lungs, nor phlegm oppressing them. When given at bed-time to the quantity of a grain, or a grain and a half, along with a little *asa fœtida*, I have frequently seen it lessen that restlessness, and those hot flushings and sick fits, which many hypochondriacal people are liable to; but after being used for some time, it loses its effect in a great measure, unless its dose be increased from time to time. It is to be observed, that if the patient be in any degree plethoric, bleeding, or some other evacuation ought to precede the liberal exhibition of *opium*; for this will make its good effects more certain and conspicuous, and will prevent in a great measure any bad consequences that it might otherwise have.

ALTHO' *opium* is often proper for quieting many nervous and hysteric symptoms, from whatever cause they may arise, yet it is peculiarly useful, when those symptoms are principally owing to an extraordinary delicacy of the nervous system.

BUT however useful *opium* may be in many cases, yet we often meet with patients who receive more hurt than benefit from it. Some are affected with an uncommon faintness and languor about the *præcordia*, or with startings; others with a sickness and vomiting, or a violent pain with cramps in the stomach, or an itching over the whole body, especially about the eyes and nose. In some few, it occasions a raving and madness.

ALTHO' *opium* in many cases exhilarates, instead of occasioning heaviness and sleep, yet it ought rarely to be given to patients who are low-spirited; for, however it may relieve them for the present; yet, after its effects are over, they generally become more depressed than before.

OPIMUM given too largely, and too long continued, lessens the sensibility and vigour of the whole nervous system; whence not only the strength of the body, but also the faculties of the mind are considerably impaired.

BUT,

BUT, notwithstanding these bad effects of *opium* when too liberally used, I have seldom seen any mischief from it, as a palliative, in disorders arising from a too great delicacy of the nerves, where it was ordered with discretion, and given in small quantities at first. Nay, in this way, those who suffer most from *opium* may be brought at length to bear it easily. A remarkable instance of which I lately had in a middle-aged lady, whom four or five drops of *laudanum*, taken by the mouth, affected with a violent pain and cramp in her stomach; and sixteen drops in a clyster, tho' it did not occasion those complaints, made her delirious for twelve hours: For this lady having afterwards begun with one drop of *laudanum*, gradually rose to twenty-five; nay, she has sometimes taken that quantity thrice a day, without feeling any of its former bad effects. In cases of great sickness, accompanied with a pain in the stomach and frequent vomiting, when the patient could not bear *laudanum* inwardly, I have ordered three or four tea-spoonfuls of it to be rubbed on the belly and region of the stomach, afterwards applying to these parts a piece of flannel moistened with Hungary water made hot. The effect was, that all the patient's complaints began to abate in less than an hour after the application of the *laudanum*, which I ordered to be repeated at the distance of six or eight hours, if it was necessary.

THERE is one inconvenience which seldom fails to attend the continued use of *opium*, viz. costiveness, which is best remedied by taking, now and then, an aloetic pill, or some other gentle purgative. But in some cases of pains in the stomach and bowels, with indigestion, much flatulence and belching, where *laudanum*, chiefly through its binding quality, did not answer so well, I have found very good effects from the *extractum hyoscyami*, given from a grain and a half to three or four grains at bed-time, and repeated in a less quantity in the morning; for altho', as an anodyne, the powers of this extract are much inferior to those of *opium*, yet, by its proving often laxative, it becomes preferable to it in several cases.

(b) SUCH as, by affecting the nerves in an agreeable manner, and perhaps relaxing them, lessen the sense of pain, and often put a stop to

to tremors, convulsions, spasms, and an uncommon agitation of the nervous system. Of this kind are the warm *semicupium*, *pediluvium*, and hot fomentations, which are frequently serviceable in cases where *opium* would be improper; but as they all tend to relax, they are only to be used by delicate people, as palliatives in urgent cases.

(c) SUCH as, by their peculiar *stimulus*, powerfully affect the nerves, so as not only to render them less sensible of the irritation arising from various morbid causes, but also to communicate to them some degree of vigour, at least for a short time.

Of this kind are camphire, castor, musk, and the fetid gums. The first and most remarkable effects of these medicines are owing to their action on the nerves of the stomach; but in what particular manner they operate on these nerves, we know not. They do not seem, at least most of them, to possess any real stupifying or narcotic quality, like *opium* and other medicines of that class.

CAMPHIRE is very volatile and penetrating; it promotes perspiration, and frequently acts as an antispasmodic; it sometimes procures sleep in fevers attended with raving, where *opium* would prove hurtful; and I have found it of good use in rendering more quiet and composed some maniac and melancholic patients.

CAMPHIRE, given in large quantities to different animals, produces sleep, sometimes madness, a vomiting, purging, a flux of urine, the hiccup, epileptic convulsions, and death*: and several of these effects are so sudden, that they must proceed rather from the
immediate

* Commentar. Bononiens. tom. 4. p. 199. &c.

The following case was some time since communicated to me by a friend.

A gentleman desirous of knowing what effects a large dose of camphire would have, swallowed half a dram of it dissolved in a little oil of olives, and very soon after perceived an uncommon but not disagreeable glow of heat in his stomach. After having walked abroad for half an hour, upon looking at a news-paper, he found himself quite incapable to understand what he read, his head being crowded with a great many confused ideas. He now began to stagger when he walked: and, some time after, a dark cloud seeming to come over his eyes, and feeling other symptoms which made him apprehend an apoplectic attack, he went to a neighbouring apothecary, with a view to get some blood taken away; but upon going into the open air, all these symptoms began to abate; and, in a few hours, he found himself in his usual health, without the assistance of any remedy.

immediate action of the camphire on the nerves of the stomach, than from its being mixed with the blood.

PHYSICIANS have differed widely in their opinions concerning the nature of camphire; some having esteemed it hot, and others of a refrigerating nature. But as it is not my purpose to enter deeply into this dispute, I shall only observe, that altho', in some cases, a glass of claret or port, or even a dram of brandy, will render the pulse slower and the body cooler; and, in catarrhus fevers, bastard peripneumonies, pleurifies, and anginas, blisters often lessen the quickness of the pulse remarkably; nevertheless, wine, brandy, and blisters, are, in their own nature, not cooling, but heating: In like manner, camphire, as its effects in the mouth and on the skin and the eyes shew, is naturally heating; but sometimes it may cool, by lessening or removing some disorder in the body which increased its heat and quickened the pulse. I have known in many cases a considerable sense of heat raised in the stomach, by a bolus of six or seven grains of camphire well mixed with a scruple of conserve of roses. However, altho' I cannot agree with those who think camphire a cooling medicine, yet I do not look upon it to be so heating as some have imagined. Perhaps camphire may excite a less degree of heat in the stomach than in the mouth, or even than when applied to the skin; for we know that the same stimulating substances affect the nerves of the stomach and of these parts very differently.

CASTOR. I cannot help thinking the virtues of this medicine, in nervous disorders, are less than many have imagined. When given from twelve to twenty grains, it sometimes procures rest, not, as I imagine, by any true narcotic quality like *opium*, but by lessening that uneasy sensation in the stomach from wind, which is often the cause of watching: And indeed, *castor* seems to have the best effects on those patients whose complaints are in a great measure flatulent. In some cases, I have thought *laudanum* had a better effect when it was joined with *castor* either in substance or in tincture. A gentlewoman aged upwards of forty, much troubled with flatulence and low spirits, was often seized, when she lay to sleep,

sleep, with a sense of faintness about her stomach, which obliged her to sit up, and often prevented her from getting rest most of the night. Twenty drops of *laudanum* made her drowsy, but did not remove the faintness; this, however, was effected by adding to it a tea-spoonful or two of the *tinct. castorei composita*.

MUSK is less heating than *castor*, and may be given in cases where neither it nor *opium* are proper. Altho' the smell of musk is offensive to many, yet I have scarce ever found it disagree with the stomach. It is chiefly useful in the *subfultus tendinum* in fevers, in the hiccup, cramps in the stomach, and other spasmodic disorders. I have tried it in the chincough and the true spasmodic *asthma*; but it was given in too small doses to determine, with certainty, as to its virtue in these diseases. Two or three grains of musk well rubbed with a little sugar, and mixed with half a table-spoonful of mint-water, will sometimes stop the vomiting occasioned by teething in children. The good effects of musk are frequently less conspicuous from its being not genuine, or taken in too small doses. Riverius mentions it as having, in his time, been given with success, to the quantity of thirteen grains, in a hysteric fit; and now it is common to order it in this, or a larger dose, three or four times a-day.

ASA FOETIDA is the strongest of the fetid gums, and almost the only one that I have been in use to prescribe internally in nervous or hysteric cases. It has good effects in flatulent disorders and spasms of the alimentary canal, and in asthmatic fits that are either owing to wind in the stomach, or increased by it. In cases where sudden relief is wanted, it ought to be given dissolved in some of the simple waters. I have often given with advantage pills of *asa fetida*, *p. 3. aloes*, and *sal. mart. ana p. 1.* to patients who, along with a costiveness, were troubled with flatulent pains working up from their bowels to their stomach, and producing sickness and vomiting. These pills were taken every night, or once in two nights, in such quantity as to keep the body gently open. *Asa fetida*, like *castor*, sometimes procures sleep; it gives relief in fits of lowness, especially when dissolved in spirits, or joined with the volatile salts: But a

too frequent repetition of such warm medicines hurts the stomach at last.

WHEN nervous or hysteric complaints are attended with a quick pulse and a feverish heat, the fetid gums, camphire, and *castor*, on account of their heating quality, ought to be given very sparingly, or not at all. They are much better adapted to cases where the pulse is low and slow. As we do not know the particular manner in which each of them operates on the nerves, so we cannot tell, before trial, in what constitutions they will severally be most successful. Frequently one of them will answer where another has failed: nay, such is the uncommon disposition of the nerves of the stomach in some cases, that a table-spoonful of the juice of lemons, unmixed with any thing, has never failed to relieve a palpitation of the heart, after many of the medicines called antihysteric had been tried in vain: And agreeably to this, we are told by Riverius, that a draught or a clyster of vinegar and water has often given immediate ease in a hysteric fit*.

IT is to be remarked, that the several medicines mentioned under this head, (2. *a, b, c,*) are chiefly serviceable as palliatives, for lessening or removing the present pain or other complaints in nervous and hysteric cases, but not for giving any durable strength to the body, or firmness to the nerves, upon which depends the radical cure. However, when those disorders do not proceed so much from a general debility of the nervous system, as from a morbid or unnatural state of the nerves of the stomach, or some other part, long-continued palliation may sometimes make a cure; for while the palliative remedies lessen the bad effects of this disorder of the nerves, Nature, either by herself, or with their assistance, at length expels or subdues the morbid cause. Thus obstinate headaches, as well as several other complaints commonly reputed of the nervous kind, have been cured, after other remedies had failed, by the long-continued use of *opium*, as will appear from the following cases, which were communicated to me by a friend.

4 N.

N. N.

* Praxis medica, lib. 15, cap. 6.

N. N. aged 28, healthy and strong, after a sea-voyage of three months, during which he was almost constantly sick at the stomach, but never vomited, was much exposed to cold in a long journey he made by land. At this time, something happened which greatly vexed him, and soon after, he began to be affected with a fixt pain in his forehead, which increasing by degrees, at last spread over his whole head. I saw him first, about two years after the headach began, at which time he complained of a constant pain, attended with a weight and heaviness in his head; he had besides sharp flying pains in different parts of it, as if a nail had been driven into them. At certain times the headach increased greatly, and was attended with a quick pulse. He frequently passed great quantities of pale water, especially in violent fits of the headach. His sleep was disturbed with frightful dreams, from which he used suddenly to awake in terror, and with a sense of great oppression. He was generally low-spirited, suspicious, and peevish, tho', on some occasions, he was uncommonly chearful. The least contradiction threw him into a fit of melancholy. He felt a tension about his eyes, especially when his head was much pained. There was scarce any secretion of *mucus* from his nose; and so moveable was his nervous system, that if he retained his water too long, or hurt his nose ever so little, by hastily bringing away from it some of the hardened *mucus*, he never failed to have an increase of his headach. He was liable to fits of sickness at his stomach, and often threw up a clear watery humour without taste or smell. He was generally costive, and his pulse good, except when attacked with the violent fits of pain in his head, which nothing was so apt to bring on as any intense thought or long-continued application of mind: *Involuntaria penis erectione, cum seminis plerumque emissionem, tam die quam noctu, saepe tentatus fuit.*

For three years after I had first seen him in this condition, he continued under the care of some physicians of character in Italy, who, having prescribed for him a variety of medicines without any advantage, gave him up as incurable. Upon this I told him one remedy still remained, which might be of service, *viz. opium*; and

as he readily agreed to my advice, I began with giving him half a grain every night at bed-time. I also dissolved two drams of strained *opium* in four ounces of spirit of wine, and ordered him to rub a little of this on those parts of his head which were most pained. The dose of *opium* at bed-time was gradually increased to a grain and a half, and sometimes he took a grain twice a-day. He had not used the *opium* a month, before he became sensibly better, and in eight or ten months found himself free from all his most troublesome complaints. After this, he began to lessen the dose of the *opium*, and to take it only once in two nights, and sometimes seldom. Only when, from vexation or any other cause, he was threatened with a fit of the headach, he immediately had recourse to the *opium* in a larger quantity. He was advised to use exercise daily, and to keep his mind as easy and chearful as possible. At first he drank a few glasses of wine at his meals; but, after he had taken the *opium* for some time, he found that a single glass of wine heated him, and made his headach worse; on which account he confined himself to water alone. The third year after he began to use the *opium*, he was so free from his complaints, that, during the space of twelve months, he did not take above three doses of it.

It may be worth while to remark, that this patient was so sensible of any change of weather, that, by a general feeling of weakness and inactivity, and of pains in his joints, he could have told, in the morning before he got out of bed, that the weather was moist and rainy, or the winds easterly or southerly.

M. N. an unmarried woman aged 30, after considerable vexation of mind, began to be seized in much the same manner with the above patient, and had taken medicines for five years to little purpose. The chief symptoms were a constant and severe pain over her whole head, especially the back-part, a stiffness in the muscles of the neck, great pain and looseness of her teeth; disturbed sleep, frightful dreams, low spirits, shakings and tremblings of her whole body, cold and hot fits by turns, flushings in her face, flatulence and swelling in her stomach, with frequent belchings, inactivity, loss of appetite, flying pains all over her body, and inability to

apply with attention to any thing serious. In summer 1759, she began to take the *opium* in the same way with the former patient. In three weeks she found herself somewhat easier, and after six weeks was much better in every respect. Her headach. was mostly gone, her teeth were free from pain, and firm, her sleep much less disturbed, and the flushings and shakings in a great measure removed. For about two weeks after she began to take the *opium*, she was troubled with gripes, which however went off after being longer used to this remedy. A solution of *opium* in spirit of wine was often applied to her head and neck, and always gave her ease.

II. WITH regard to the second intention of cure, which was to correct or remove the occasional causes, which, especially in such as are predisposed, give rise to all the nervous, hypochondriac, and hysteric symptoms; as these causes are various, the medicines must be often different: Nay, what is proper in one case, may be hurtful in another.

THE occasional causes were distinguished before into the general and particular.

THE general causes were,

1. SOME morbid matter bred in the blood.
2. THE diminution or suppression of some habitual evacuation.
3. THE want of a sufficient quantity of blood.

THE particular causes were,

- | | | |
|---|---|-------------------------------|
| 1. WIND | } | in the stomach
and bowels. |
| 2. TOUGH phlegm | | |
| 3. WORMS | | |
| 4. ALIMENTS improper in their nature or quantity. | | |
| 5. OBSTRUCTIONS, frequently of the scirrhus kind, in the abdominal <i>viscera</i> . | | |
| 6. SUDDEN and violent affections of the mind. | | |

IN order therefore to treat distinctly of the second intention of cure, it will be necessary to mention particularly the different remedies

medies which are most likely to lessen or remove these several causes.

1. SOME morbid matter in the blood.

(a) As we are often ignorant of the nature of that matter in the blood which is the cause of nervous disorders, so we must be often at a loss how to correct or expel it. When I suspect it to be of that kind which produces the *arthritis vaga*, from knowing the family-distemper of the patient, his constitution, and manner of life, or his being much troubled with flying pains in his head, arms, or limbs, I rely most upon a proper diet and exercise, with the tincture of the bark and bitters mentioned under the first intention of cure, in order to prevent the generation of this matter, or gradually to subdue and carry it off, when already generated. But supposing the bark and bitters had no power to destroy the arthritic matter in the blood, which seems most probably to be the case, yet by strengthening the stomach and bowels, they may not only retard the generation of more, but prevent, in a great measure, an attack upon these parts, which are observed to suffer most, when, from any cause, they have been much weakened, or otherwise put out of order.

THE reputation which bitters have had in gouty cases among the antient, as well as some of the modern physicians, led me to think, that a well chosen medicine of this kind might be very useful in nervous, hypochondriac, or hysterical complaints from an arthritic matter in the blood: And altho', in patients in the decline of life, the tincture of the bark and bitters has often failed me, yet in those who were under forty or fifty, I have found it do more service than any other remedy.

WHEN the patients are liable to fits of the true gout, I increase the proportion of the *rad. gentian.* and *cort. aurant.* in the tincture, adding at the same time some nutmeg or ginger, especially if the stomach be cold and flatulent. In this case also, the tincture may be taken to the quantity of two table-spoonfuls twice a-day.

I have known an indigestion and flatulence, with a pain and sickness at the stomach from the gout, greatly relieved after other medicines had failed, by drinking thrice a-day six ounces of a strong decoction of several of the common bitters * in water: And a gentleman of my acquaintance, who had been much troubled for fifteen years with a pain in his stomach, has been cured by chewing two drams of the roots of gentian daily. This kept his body open, and increased his appetite; it began to give him ease in a few days; and when, upon omitting it, the pain returned in a lesser degree, it was quickly removed by having recourse to the gentian again.

A milk diet, which sometimes has proved a radical cure for the gout †, has been commended by Sydenham in certain hystERIC cases, after other medicines have failed ‡. I can say little of its effects in either case from my own experience. We meet with few patients who will confine themselves to this diet, and in several cases it is improper to advise it. About eighteen years ago, I had a patient aged 48, who, on account of an ulcer in his lungs, restricted himself for many months to a diet of milk and vegetables alone, and after he got free from that disease, continued to live in this way for several years. This person, who was of a very full habit, and had been formerly attacked once a-year at least with the gout, remained free from this distemper for seven or eight years, that is, till some years after he had returned to the use of flesh-meats and fermented liquors.

LIME-WATER is said to have been drunk successfully by several gouty patients §. I have only had one who gave it a decisive trial. This person was aged about fifty, and had for several years been subject every winter to a smart fit of the gout. In February or March 1758, he began to drink daily an English quart of lime-water, living at the same time very temperately. Before the end of the first year, he had a very slight attack of the gout: about the

end

* Viz. Rad. gentian. calom. aromat. cort. aurant. summit. absynth. centaur. min. card. benedict. with sem. carv.

† Celsus de medicina, lib. 5. cap. 24.

‡ Dissert. Epist. ad D. Cole.

§ See Alston's Dissertation on quick lime and lime water, part 3.

end of the second year, he had rather less of this disease; but after he had continued drinking the lime-water constantly for near three years, he was seized with a severe and long-continued fit of the gout in both his hands and both his feet. This patient observed, that the lime-water, when drunk warm, mended the state of his stomach, when it was disordered before the coming on of a fit of the gout, and he thought it had a good effect in driving this disease to the extremities. The lime-water agreed perfectly well with him, and mended his appetite.

FROM this case it may be fairly concluded, that lime-water does not radically cure the gout, or destroy the arthritic matter in the blood, although, by strengthening the stomach and intestines and preventing acidity in them, it may render the attacks of this disease less frequent, and in some persons, perhaps, less severe*.

WHEN lime-water is drunk for the cure of nervous complaints from an imperfect gout, it ought to be taken to the quantity of at least an English quart daily; as, at first, it is sometimes apt to occasion an uneasy heat in the stomach, a little sweet milk may be added to it; but afterwards it is better to drink it alone. In the winter-season, and when the stomach is more disordered than usual, the lime-water ought to be drunk nearly blood-warm.

SOAP has been proposed by the late Doctor John Clerk, a physician of distinguished character in this place, as the proper solvent of the arthritic matter in the blood†. It has sometimes been of use in old rheumatisms, and may be properly taken along with the lime-water, as it prevents costiveness and destroys acidities in the stomach and bowels.

As

* It may be proper to mention, that a patient of Dr Clerk's, physician to the Royal Infirmary here, who used to have a severe and long-continued fit of the gout once in two years, has been kept free from this disease for near three years past, by drinking off, at once, an English quart of lime-water, every forenoon about eleven o'clock. The lime-water taken in this way, always purges him twice or thrice about three o'clock in the afternoon. But as this person is of a very full habit of body, it is probable that the lime-water has proved useful to him, rather by that daily evacuation which it occasions by stool, than by any virtue it possesses of destroying the arthritic matter in the blood.

† See Dr Pringle's Observat. on the diseases of the army, part 3. chap. 2. edit. 1.

As some persons subject to the true gout have found great benefit from drinking, twice a-day, about a gill and a half of a strong infusion of tanfy in boiling water, it is probable the same medicine might be useful in those complaints which arise from an imperfect gout affecting the stomach and other parts. But of this I can say nothing certain from my own experience, not having had any patient who gave the tanfy a fair trial.

Issues and perpetual blisters have been often of use in headaches, and in the sciatic or chronic rheumatism affecting one leg; but I have not found them do much service in nervous or hypochondriac complaints from an arthritic humour.

(b) I have observed above, that complaints of the nervous kind sometimes proceed from that kind of humour in the blood, which is commonly, but improperly, called scorbutic; and which, when it is thrown out on the skin, appears in the form of tetters, scurfy eruptions, or the *lepra Græcorum*. In this case, we must endeavour to drive the morbid humour outwards to the skin, by vomits, warm stomachics, and sudorifics; after which the radical cure must be attempted by mild mercurials, and the purging mineral-waters.

The method which I have always found successful, at least in slighter cases, is to give twelve grains of the *pilula Æthiopicae* every night at bed-time, and every other morning a dram or a dram and a half of polychrest salt dissolved in an English pint of water*. The salt, besides otherwise contributing to the cure, opens the body, and prevents the pills from raising a salivation, which they are sometimes apt to do. These medicines are to be used till the scurfy or leprous eruptions quite disappear. When the obstinacy of the disease requires it, I give the pills both morning and evening.

ALTHOUGH the true scurvy is a disease rarely observed, except in those who live at sea, or in marshy places; yet we frequently meet with patients who have some degree of a scorbutic taint in their

* In place of this solution of the polychrest salt, I have sometimes ordered sea-water to be drunk.

their blood, as appears from their spongy gums, a lassitude, and other complaints. I have had several patients of this constitution, who were deeply affected with the hypochondriac disease; their chief symptoms were low spirits or melancholy, watching, flatulence, frequent spitting of the *saliva*, a bad digestion, flying pains, a tightness about the *præcordia*, a dark colour and troubled look. I have never succeeded in curing any of those patients; but a long course of the tincture of the bark and bitters with elixir of vitriol, and daily exercise, seemed to agree better with them than any thing else. When they are costive, I order, once in two or three days, as much soluble tartar as is necessary to open them gently.

(c) WHEN nervous symptoms are occasioned by some morbid matter remaining in the blood, in consequence of some former disease imperfectly cured; we must have recourse to such remedies as are best suited to the nature of that disease or the circumstances of the patient.

THAT humour which produces the rash or miliary eruption, when it falls on the internal parts, instead of being thrown out upon the skin, generally occasions a great depression of spirits, anxiety, and faintness, pale water and watching, and sometimes raving and convulsions. In this case, I have found most advantage from the warm *pediluvium*, or warm fomentations applied to the feet and legs, from blisters, wine, whey, and boluses of camphire, saffron, and salt of hartshorn. When in the miliary fever, the patients are much oppressed at the stomach, and complain of a difficulty of breathing, a gentle vomit of ipecacuanha, or of an infusion of camomile, often gives relief.

THE warm *pediluvium* and fomentations often procure sleep, and give some immediate ease to the patient; they likewise contribute to promote the miliary eruption, by removing that tension or spasmodic contraction of the cutaneous vessels which frequently retards it. Where the patients are in any degree plethoric, bleeding will often not only give some present relief, but, by relaxing the vascu-

cular system, will also contribute to the expulsion of the morbid matter by the skin.

2. WHEN nervous, hypochondriac, or hysteric symptoms proceed from a diminution of some habitual evacuation, that evacuation is to be promoted by the proper remedies.

(d) WHEN the *menfes* are obstructed, we must endeavour to recal them, and, till that can be done, the most troublesome symptoms are to be palliated. There are few cases in which we are oftener disappointed than in bringing back the monthly evacuation, after it has been long suppressed; and the medicines proper in one case may prove ineffectual or even hurtful in another.

WHEN the want of good blood is the cause why the *menfes* do not flow, the best remedies are the bark, bitters, and steel, together with a nourishing diet, and exercise. After the patient has, by these means, got more and better blood, it ought to be determined to the *uterus* by frequent doses of *tinctura sacra*, and by making the patient sit every evening over the steams of warm water.

IF a *plethora* or a too great abundance of blood prevents the flux of the *menfes*, bleeding, especially in the foot or at the ankle, and gentle purges, will prove most effectual.

WHEN the thickness or viscosity of the blood hinders it from making its way through the uterine vessels, frequent vomits, and the *pilule mercuriales laxantes*, or gentle purges with calomel, will answer best.

LASTLY, when the suppression of the *menfes* has been owing to a spasmodic contraction of the uterine vessels, in consequence of cold, some violent passion, or other causes, the chief remedies are the warm *semicupium* and *pediluvium*, oily draughts, and pills of aloes, *asa fetida*, extract of black hellebore, and saffron. A clyster of warm water, with thirty or forty drops of *laudanum*, may be given, in the evening, about the time the *menfes* should return.

OBSTINATE obstructions of the monthly evacuation in women have sometimes been cured by electrifying them, and drawing the sparks chiefly from their thighs. But Dr Clerk informs me, that
he

he has observed this remedy to succeed best in those whose pulse was small and languid.

SOME young women, about the time of the return of the *menfes*, are apt to be seized with violent pains in their back and belly, with faintings, raving, and sometimes convulsions. In such cases, the warm *semicupium* is of great use; but as often this cannot be readily got, I have generally ordered, with success, a clyster of warm water with fifty drops of *laudanum*, and a flannel bag with emollient herbs to be wrung out of hot water, and applied to the *abdomen*. When the patient has been costive, a laxative clyster with *asa fœtida* must be given to procure a stool, before the anodyne one is injected.

IN the intervals between the returns of the *menfes*, in order to render the patient less liable to the above mentioned complaints, I have advised, with good effect, the frequent use of the warm *pediluvium*, some doses of the *pilule rufi*, and those oily draughts, which, in this case, were much commended by Sir David Hamilton*; and which I have also found of good use in pains of the bowels, in those whom the *menfes* had left.

I have sometimes met with unmarried women, who were liable to be attacked with faintings and convulsive fits, after every period of the *menfes* was over; which seemed to be owing to this evacuation being less copious than usual. In a case of this kind, the following remedies used for two or three months proved successful.

R. Aloes socotin.

As fœtid.

Extract. hellebor. nigr.

Sal. Mart.

Croc. Angl. ana drach. i.

Elix. proprietat. q. f. ut. f. pil gr. iv.

Quarum capiat. v. vel vi. alternis noctibus.

R. Rad. Gentian.

Calam. aromat. ana unc. i.

Summit. centaur. min. drach vi.

Flor. anthos, drach ii.

M. f. materialia infundenda, per hor. vi. in aqu. bullient. lib. iv. colatur. adde

Tinct. Cort. Peruvian. unc. x. Misce.

Cape unc. iii. bis in die.

4 O 2

ALONG

* De praxeos regulis, cap. 3.

ALONG with these medicines the *pediluvium* was used every night at bed-time.

WHEN, in the decline of life, the *menfes* cease, various nervous or hysteric symptoms appear, which are generally lessened, and sometimes removed, by frequent small bleedings, gentle stomachic purges, and issues.

(b) IF the hæmorrhoidal flux is wanting in those who have been accustomed to it, we must endeavour to recal it by emollient fomentations, and aloetic medicines. When these or other remedies prove ineffectual, Hoffman has advised leeches to be applied near the *anus* once a month.

(c) WHEN old ulcers, or sores too quickly dried up, have given occasion to nervous disorders, purgatives, and especially issues or a seton, will be most successful in carrying off that humour which disturbs the body.

(d) WHEN pimples or other eruptions on the face have been suddenly repelled by improper applications, violent headaches, giddiness, sickness at the stomach, palpitations, and other nervous symptoms have been sometimes the consequence. In such cases, if the morbid humour cannot be brought back to the face, we must try to carry it off by perpetual blisters or issues in the head or neck, and by mercurial purges.

3. WHEN nervous or hysteric complaints are occasioned by a want of blood, in consequence of an immoderate flux of the hæmorrhoids, *menfes*, or *lochia*; the cure consists in restraining these evacuations, and filling the vessels by means of such aliments as are light and nourishing, but not heating. In the mean time, the violence of the symptoms must be abated by anodynes and wine, or other cordial medicines. A horizontal posture is here of considerable use.

THE medicines which I have found most successful in restraining an immoderate flux of the *menfes*, are the *tinctura rosarum*, *terra Japonica*, alum, *opium*, and elixir of vitriol.

I have sometimes given the alum mixed with *terra Japonica* as in the *pulvis stypticus*; but of late I have prescribed it more frequently

ly in the following form, as being less disagreeable to the stomach.

R. Lact. recent. bullient. lib. i.
 Alum. Rup. pulverat. drach. i. ad drach. i. fs. Misce
 ut fiat coagulum; et fero colato adde
 Sacchari albi unc. i.
 Cape unc. iii. quater in die.

IF the alum-whey occasions a sourness in the stomach with a *cardialgia*, a scruple of crabs eyes or prepared oyster-shells, twice or thrice a-day, will be useful. In one case, the alum-whey lessened a *profluvium mensium* after the patient had taken, for some time, forty drops of the *tinctura antiphthifica* thrice a-day, without any benefit. The same medicine also cured a *fluor albus* of several years standing.

I have not observed remarkable effects from the bark, in stopping hæmorrhages. After an immoderate flux of the *menfes* had resisted that medicine taken in substance for near a fortnight, I have seen it yield in two or three days to such a mixture as the following.

R. Aqu. menth. unc. vi.
 Cinnamom. f. v. unc. ii.
 Confect. Japon. drach. vi.
 Syr. limon. unc. ii. Misce.
 Cape cochl. ii. 4ta vel 6ta quaque hora.

To remedy the costiveness which this mixture generally occasions, it becomes necessary once in two or three days, to order either some rhubarb or a laxative clyster.

THE bark, which is more remarkable for its strengthening than astringent quality, seems to be less adapted for stopping hæmorrhages, than for restoring strength to those who have been reduced by them. However, it is often very proper, not only to give the bark after the flux of blood is lessened, but also, at the same time, with some of the stronger astringents.

WHEN a *profluvium mensium*, or a flooding after abortion, is attended with, or preceded by an acute pain, not inflammatory, in the lower part of the back or belly, and returns with greater violence; as often as the pain returns or increases, *opium* will prove a
 more

more effectual remedy than any of the astringents, as happened in the following case.

MRS D——, aged between 30 and 40, having gone abroad too soon after an abortion in the fourth month of her pregnancy, was seized with a violent pain in her back and the lower part of her belly, which returned once in eighteen or twenty hours, and was always attended with an excessive flooding, which abated when the pain left her. Having been called, after she had used several astringent and strengthening medicines with very little advantage, I ordered a clyster of six ounces of an infusion of dried red roses in boiling water with fifty drops of *laudanum*, to be given every night at bed-time, and once in two days a laxative clyster in the morning, if it should be necessary. After the first anodyne clyster, she had little either of the pain or flooding, and after the third, was quite cured of both these complaints.

LUDOVICUS SEPTALIUS *, and after him, Sir David Hamilton †, has commended a strong decoction of bitter orange-skins as a most effectual remedy in a *profluvium mensium*; and I have been informed by an able physician, that he has prescribed it once and again with success in the following manner:

R. Cort. aurant. Sevil. recent. integr. vii.
Coque ex aqu. fontan. lib. iii. ad lib. ii.
Colaturæ adde sacchar. alb. unc. i.
Elix. vitriol. gutt. lx.
Cape cochl. vi. tertia quaque hora.

I have known the *fluor albus* cured, in a great measure, by a course of sea-bathing, after many powerful medicines had been tried in vain. The same remedy, in the intervals of a *profluvium mensium*, has contributed much to lessen that flux; and a lady aged between 40 and 50, a patient of mine, who was so much distressed with the bleeding piles, that she rarely went to stool without losing a great deal of blood, found more benefit from sea-bathing than from any thing else. It not only lessened the discharge of blood from the hæmorrhoidal vessels, but soon gave her a better appetite, more strength, and a fresher colour.

AND

* See Animadvers. med. lib. 7. art. 144.

† De praxeos regulis, cap. 3.

AND thus much may serve for the cure of the general occasional causes of nervous, hypochondriac, and hysteric disorders: I come next to mention the method for lessening or removing their particular causes, viz.

1. WIND in the stomach and bowels.

As this proceeds either from a debility or spasmodic affection of the alimentary canal, or from improper aliments; the remedies for performing the radical cure may be found under the first intention of cure above *, and below under N^o 4. where the treatment of nervous complaints, arising from errors in diet, is laid down. The medicines proper for giving immediate relief for the uneasy sensations occasioned by flatulence, will be mentioned afterwards, when I come to treat of the cure of some of the principal symptoms of the nervous or hysteric kind.

2. TOUGH phlegm bred in the stomach and intestines.

THE cure of this phlegm is often tedious and difficult, and, in many cases, can by no means be obtained: For altho', by repeated vomits, we may clear the stomach of the present load; yet, unless that organ is sufficiently strengthened and its secretory vessels restored to a sound state, more phlegm will be continually produced. Wherefore, besides frequent vomits, we must have recourse to the bark, bitters, chalybeats, animal food, and exercise, especially riding or sailing †. Repeated doses of the *tinctura rhabarbari amara*, or *elixir sacrum*, are not only useful for strengthening the stomach and bowels, but for carrying down and evacuating part of the phlegm that disorders them. I have sometimes thought that the *emplastrum stomachicum* applied to the epigastric region was of use.

THOSE who are apt to breed much phlegm in their stomach, generally find it necessary to take a vomit once in ten days or a fortnight, and sometimes oftener. When a vomit of *ipecacuanha* is taken,

* See above, page 633, &c.

† Si vero pituita stomachus impletur, utilis navigatio. Celsus de medicina, lib. 4. cap. 5.

taken, either an infusion of horfe-radish should be drunk, or a little brandy or powder of mustard should be added to each draught of the warm water; for these, by their warm *stimulus*, tend to invigorate the stomach, at the same time that the phlegm oppressing it is evacuated.

As lime-water dissolves *ichthyocolla*, and other glutinous substances, I thought it might be worth while to try what effect it would have on the tough phlegm bred in the stomach. With this view, I poured three gills of lime-water on a gill of that phlegm newly vomitted up, and mixed them well together: at first, the phlegm seemed to be rendered somewhat thicker by the lime-water; but, after standing five or six hours, it was quite dissolved. After this, one of my patients, at my desire, mixed one part of very tough phlegm, brought up from his stomach by a vomit in the evening, with two parts of lime-water; and upon examining this mixture next morning, he found the phlegm had wholly lost its tenacity. This gentleman, at the same time, mixed some of the phlegm with common water; but after standing 24 hours, it retained its tenacity in a great measure, altho' it was rendered thinner by the mixture of the water.

WHEN lime-water is used with a view to the cure of phlegm in the stomach, it should be drunk to the quantity of near an English pint, every morning upon an empty stomach, and nothing should be taken for two hours after. An hour and a half before dinner, and as long before supper, half a pint should be also drunk.

FURTHER, as often as an emetic is used, the patient, some time after its operation is over, should first of all take a draught of lime-water, which in this case will act more strongly in dissolving any phlegm that may remain in the stomach, as well as in bracing its relaxed pores and vessels.

WHEN, together with a tough phlegm, there is a considerable degree of acidity in the stomach, I have known good effects from ten grains of the *sal absynthii* or salt of tartar given twice a-day. When the stomach is quite free from acidity, the elixir of vitriol may be
of

of use to strengthen its vessels, altho' it has no effect in dissolving the phlegm.

3. WORMS in the stomach and intestines.

IN this case, while we palliate the most troublesome symptoms, we must endeavour to destroy the worms by such anthelmintic medicines as may seem best adapted to the particular state of the patient. I shall only add on this head, that, in some cases, I have seen good effects from an infusion of the root of the Indian pink *; but this remedy is certainly much less efficacious here than in South Carolina, and seems to lose a great deal of its virtue by being long kept.

I have ordered, with good success, to some grown persons, six drams or an ounce of Spanish soap daily. It destroys the *ascarides* as well as the round and flat worms. Lime-water has been much commended as an anthelmintic, but it will scarcely be of any use, except when the worms are lodged in the stomach, or high up in the intestines; for, if they remain in the *ileum* or the inferior part of the *jejunum*, the lime-water will be mostly all absorbed before it can reach them.

4. ALIMENTS noxious from their quality or quantity.

WHEN nervous, hypochondriac, or hysteric disorders are owing to this cause, or increased by it, a proper regulation of diet is the principal remedy.

(a) IF, by a long habit of eating too little, the concoctive powers are much weakened, the patient must, by slow degrees, increase the quantity of his aliment.

IF, on the other hand, his complaints have been occasioned by excess in eating and drinking, he must gradually lessen the quantity, till he has reduced himself within the bounds of strict temperance; that is, he must never eat so much at dinner as to make himself, soon after, unfit to go about any business, or apply himself to any study;

study ; and he must make light suppers, or none at all, if he does not find his dinner digested.

I have known some people much afflicted with the gout, while they lived too fully, who being afterwards reduced, by necessity, to a spare diet, got quite free from that distemper: And indeed, when nervous ailments have been owing to high living, or an arthritic matter in the blood, abstinence, or rather moderation in eating or drinking, is of the greatest consequence in the cure.

(b) WITH regard to the quality of the food, the patients ought to abstain from all heavy and fat meats, from whatever they find hard of digestion, and from all flatulent aliments.

IF the stomach and bowels have been hurt by a flatulent diet; greens, roots, fruits, and whatever is apt to breed much wind, ought to be avoided; and the patient should live chiefly on bread, rice, and flesh-meats, with a few glasses of wine of a good body and age, and not apt to turn sour.

IF heavy meats, rich fauces, and the too free use of wine or other strong liquors have hurt the stomach and bowels, the patient ought gradually to reduce himself to a small quantity of wine, and eat only the lighter animal substances plainly dressed, and such vegetables as are least flatulent. In this case, a diet of milk and vegetables alone may sometimes be of great service, which, however, must not be gone into all at once, but very gradually: And it is further to be observed, that while some, who had been accustomed to animal food and wine, have found great benefit by abstaining from them, without losing much strength, or any spirits, there have been others of a different constitution, who could not bear the want of such a diet; and, when wholly confined to milk and vegetables, were not only troubled with faintness and lowness of spirits, but with great flatulence and other disorders of the *primæ viæ*: From which it may appear, how far some have erred in recommending, without sufficient restriction, a diet of this kind in the greatest part of nervous disorders.

IT may be observed, that many people who have weak or windy stomachs, especially such as are liable to the gout, find not only vegetables,

getables, but milk to agree best with them, when they take at the same time some pepper or other spicery: And I have known some persons subject to violent attacks of the gout in their stomach, who have been much the better for swallowing, every morning, twelve or sixteen corns of white pepper, with the water-gruel which they took for breakfast.

THAT abstinence from wine and flesh-meats, and a diet wholly of milk and vegetables, does not prevent nervous ailments, we have a strong proof in the poorer sort of the country-people of North Britain, who, tho' they live on milk, whey, barley, pease, and oatmeal, with coleworts, potatoes, and other vegetables, without almost any animal food or fermented liquors, are nevertheless remarkably subject to pains in their stomach and bowels, flatulence, and other complaints of the hypochondriac or hysteric kind connected with it.

NAY, however much a milk and vegetable diet may be of use in some cases, to lessen or remove such disorders as have been the consequence of high living; yet, in general, it is certain that a diet of this kind is more apt to produce flatulence in the first passages and all the troublesome symptoms depending upon it, than a diet consisting partly of vegetable and partly of animal food. Nay, even milk itself, which holds a kind of middle place between vegetable and animal substances, has been observed by Hippocrates to be hurtful to those who are much subject to wind in their bowels*.

BECAUSE a mixture of flesh-meats with vegetable substances and water, kept in a heat equal to that of the human body, has been observed to ferment sooner and much more briskly than those vegetables and water alone, some have concluded that vegetable and animal aliments together will produce more flatulence in the *primæ viæ* than vegetables alone: But it ought to be considered, that the digestion of the aliments is very different from that change which happens to them in a chymical vessel; and that, as the production of flatulence in the stomach and bowels is chiefly owing to a weakness of these parts, a disordered state of their nerves, or spasmodic contractions in them; a certain proportion of animal food, by in-

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vigorating

* Aphor. sect. 5. No. 64.

vigorating the alimentary canal, gratefully affecting its nerves, and rendering it less liable to irregular motions and spasms, may occasion less flatulence in time of digestion, than would happen from vegetables alone.

WHEN nervous complaints are owing to an arthritic matter, a diet of milk and vegetables, if the stomach can bear it, may, by destroying, or rather not furnishing fresh supplies of that matter, effect a cure. But where the stomach, from its weakness, or the peculiar disposition of its nerves, cannot bear the greatest part of vegetable aliments, such a diet would be extremely improper; whilst the lighter animal food, in such quantity as can be easily digested, will not only nourish and strengthen the body more, but will act as an anodyne in preventing or allaying many complaints of the stomach and bowels.

UPON the whole, no constant rule can be given as to the kinds of food; for while a diet chiefly of flesh-meats answers well with some, others live best on milk and vegetables, either alone or with a small proportion of animal food. In like manner with regard to liquor, some cannot do without wine, while water alone, or water with a little brandy or rum, agrees best with others. Every valetudinary person ought, therefore, to keep by those kinds of meat and drink which he finds by experience to be lightest and most agreeable to his stomach. But whatever aliments may be used, moderation should be constantly observed, as people are generally less hurt by the quality than by the quantity of what they eat and drink.

(c) WHEN the stomach and intestines have been much hurt by high living, or weakened by flatulent food; besides a proper diet, the bark, bitters, chalybeates, and exercise*, will be often necessary for giving new strength to the alimentary canal. Gentle vomits and stomachic purges may also be of use to cleanse the first passages, and promote the natural secretions there.

5. INDOLENT obstructions, chiefly of the scirrhus kind, in some of the abdominal *viscera*.

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* See the first intention of cure, p. 633. &c. above (a) (b) (c) &c.

OBSTRUCTIONS of the indolent kind have their seat either in the secretory tubes of the glands, or in other vessels smaller than those which carry red blood in the glandular follicles, or in the spaces of the *tela cellulosa*, in which there is deposited, by the exhaling arteries, a fluid which soon becomes too thick to be taken up by the absorbent veins, and is daily increased by the addition of new matter of the same kind. In some cases, the vessels of the obstructed part are so changed from their original state as to separate from the blood fluids, which, by stagnating in the follicles or cellular spaces, acquire a cartilaginous nature.

IT is generally difficult to discover when nervous or hypochondriac complaints are owing to scirrhus or other indolent swellings in the coats of the stomach and intestines, or in the other abdominal *viscera*, unless when the tumours can be felt, which is seldom the case. But when I meet (especially in women after the *menfes* have left them) with complaints of want of appetite, indigestion, vomiting, flatulence, and pains in the belly which have continued long, without any considerable intervals of ease, and instead of yielding to medicines, become worse, I suspect some fixed obstruction in the stomach, intestines, or neighbouring parts, especially if the patient has a quick pulse, without any considerable heat or thirst.

WHEN hypochondriac or hysterical ailments are owing to indolent obstructions, we must endeavour to resolve them by degrees, and, in the mean time, palliate the most troublesome symptoms occasioned by them.

(a) THERE are few remedies of greater service in obstructions of the indolent and cold kind than gentle friction. It not only promotes the circulation through the small vessels, but tends to attenuate and increase the absorption of the matter stagnating in the follicles, or extravasated in the spaces of the cellular membrane of the obstructed part. I have had instances of incysted tumours cured by long continued friction alone. One was on the upper eyelid, about the size of a common cherry and of the steatomatous kind. Another was situated in the *membrana adiposa* on the left side of the abdomen;

abdomen; it was bigger than one's fist, pretty soft, and felt like the steatomas or atheromas. It was treated with nothing but friction of the part twice a-day with *oleum camphoratum*. For the first four months it did not seem to yield, but soon after it began to lessen and went off very fast. The small tumour on the eye-lid was rubbed only with the *saliva*.

(b) WARM fomentations are of great use; they not only relax the vessels and attenuate the obstructing matter, but by their warmth promote the circulation of the fluids through the obstructed part. They will often either resolve indolent swellings, or bring them to a suppuration, when internal medicines, without their assistance, would do little. They ought to be applied every morning and evening for near two hours, but should not be so hot as to be in hazard of inflaming the skin or making it too tender. I generally use flannel-cloths wrung out of hot water alone; and sometimes, in place of this, a hot decoction of wormwood and camomile-flowers, or of the tops of hemlock, adding to it a little vinegar.

IT is obvious, that when obstructions are deep-seated in the *abdomen*, neither frictions nor fomentations will have such remarkable effects as when they lie in the *tunica cellulosa* immediately below the skin.

(c) GENTLE vomits and purges * frequently repeated, are particularly useful in beginning indolent obstructions of the abdominal *viscera*. But when an obstruction in the stomach is so far confirmed as to be irresolveable, vomits, especially of the stronger kind, may prove hurtful by irritating the infarcted part, or even bursting some of its vessels †. And here it may be remarked, that many of those hot and acrid medicines commonly prescribed in nervous disorders must be likewise improper in this case, since by their stimulating quality, they will be more apt to inflame and exasperate, than to lessen or resolve any scirrhus obstruction.

(d) WITH

* I look upon the good effects of sea-water in glandular swellings to be chiefly owing to its purging quality. When it does not prove laxative, but makes the patient thirsty and hot, no good is to be expected from it.

† See above, p. 575.

(d) WITH regard to those internal medicines commonly called deobstruent, they can have little or no effect, when the obstructing matter is accumulated in the spaces of the *tela cellulosa*, and are, therefore, chiefly useful in those obstructions whose seat is in the follicles of the glands, or in the small vessels themselves.

THE internal deobstruent medicines which I have used with most advantage, are the *tartarus solubilis*, *sal polychrestus*, mercury, and soap.

I give the soluble tartar from drach. i. fs. to drach. iii. or half an ounce, and the polychrest salt from scrup. ii. to drach. i. fs. dissolved in an English pint of water, which is to be drunk at three or four draughts, every morning for two months or longer.

I commonly prescribe mercury as a deobstruent, either in the form of the *pilule mercuriales laxantes*, or of the solution of the corrosive sublimate. To prevent these medicines from running too much to the mouth, I give the pills only once in two or three days, and when the solution is used, I order a gentle purgative once in four or five days.

IN glandular swellings of the neck, of the strumous rather than the true scirrhus kind, I have seen nothing succeed so well as a course of the bark, in substance or decoction, for several months; giving, at the same time, every fourth or fifth night, such a dose of calomel and rhubarb, or of the *pilule mercuriales laxantes*, as may purge the patient twice or thrice next morning. Nor have I found these medicines less efficacious, when, together with indolent swellings and a scrofulous habit, there was a considerable degree of fever; as in the following case.

A child aged 7 years, of a lax and scrofulous habit, in March began to be affected with hard swellings on her left wrist and one of her legs, and with a soft œdematous swelling of her feet and hands; at the same time her tongue was foul, her pulse quick, and her skin hotter than natural. In June following, when I was first called, all these symptoms were increased, she was much emaciated, and her pulse beat above 130 times in a minute. As she had used many other remedies without advantage, after a vomit and a gentle
purge,

purge, I prescribed a decoction of the bark, with some spirit of vitriol, to be taken four times a-day, in the quantity of two or three table-spoonfuls ; and once in five or six days a dose of rhubarb with calomel. In less than four weeks after she began this course, her pulse became slower, her skin cooler, and her appetite better ; and at the end of two months she was almost quite free from all her complaints.

WHEN glandular swellings lie immediately under the skin, the mercurial ointment rubbed into the part, or a strong mercurial plaister applied to it, has sometimes made a cure. A gentleman, aged 21, had one of the conglobate glands on the left side of his neck swelled from cold. This swelling, which was without pain, increased gradually ; so that, at the end of three months, it had acquired the size of a hen's egg cut longitudinally through the middle. After he had used, for six weeks, mercurial purges, fomentations, and the common discutient plaisters, to no purpose, the *emplastrum mercuriale cum triplice mercurio* was applied to the part. In two or three days after, he began to salivate, and for a week continued to spit at the rate of an English pint a-day. After this, the spitting decreased gradually, and left the tumour reduced to one third of the size it had before. The warm weather of summer, which soon followed, carried off what the mercurial plaister had left unresolved.

AMONG the deobstruent medicines, Spanish soap deserves a principal place. Obstinate glandular swellings have sometimes yielded to it after mercury had been tried in vain, as will appear from one of the following cases. It should be given daily from half an ounce to an ounce or more, if the patient's stomach can bear so much.

1. A. M. upwards of 20 years of age, applied to me in April 1752 for a swelling in the epigastric region, a little below the *cartilago ensiformis*. This tumour was near as large as one's fist, and felt hard, but without pain : It was evidently under the muscles and *peritonæum*, and as it shifted its place upon the patient's turning from one side to the other, I conjectured its seat to be in the

omentum

omentum, especially as it was attended with no disorder in the stomach or bowels.

I advised him to let warm water fall from a considerable height upon the swelling; to cover it all day with a piece of flannel, to use the *pilule scilliticæ*, and drink with them at least an English quart of cow-whey daily. Some time after, he took pills of *gum ammon. galban.* and *aloes*, but without any benefit; for the tumour became larger; and when he sat to write, which his business often obliged him to do, he suffered much uneasiness from that posture. On this account I sent him to the country in the end of July, and advised him to swallow every day from half an ounce to a whole ounce of Spanish soap, and continue the whey. Towards the end of October he returned to town with the tumour sensibly diminished, and by going on with the soap till about the beginning of January, it was scarcely to be felt. He then left off all medicine, and has been ever since in good health, without any sensible swelling or hardness about the part first affected.

2. A gentleman, aged 33, after having been subject for some years, to rheumatic pains, observed in March 1752, an indolent glandular swelling, neither soft, nor yet of the hardest kind, on the right side of his neck, immediately above the clavicle. In the autumn following, having exposed himself to cold and wet on a journey, this swelling became soon after considerably larger. He then lost some blood, which was very fizy; and in November he used some warm discutient fomentations, and the mercurial laxative pills. These last, which he took once in two days for about three weeks, made him spit gently, but did not diminish the tumour. About a fortnight after he had discontinued the pills, he began to take three drachms of soap daily, and soon doubled that quantity. In three weeks the swelling being sensibly diminished, he was encouraged to continue this medicine; but, about the middle of January, having caught cold, he was seized with a *diarrhæa*, and obliged to omit the soap for above a fortnight. In February, soon after the *diarrhæa* left him, he began to be troubled with a violent itching over his whole skin, especially when in bed, and this symptom in-

creasing, towards the end of this month he was advised once more to discontinue the soap. At this time the tumour was reduced, at least one half, since the middle of December.

ON account of the increase of this itching and other complaints, he never returned to the soap; but after trying a variety of other medicines, and the air of different climates in vain, he died in August 1754.

SINCE people affected with the stone often take soap to a greater quantity than this patient did, without any complaint of itching, I think this symptom cannot be justly ascribed to that medicine; especially as the patient had nothing of it for the first five weeks he used the soap, and as it came on after a *diarrhœa* occasioned by a cold. Neither can I think the swelling in his neck was critical, and that the itching and other bad symptoms were owing to the matter in it being dissolved by the soap, and carried into the blood; because, in March 1752, when this tumour began, the patient had no particular complaints; and in November, when it was become so large as to be broader, tho' not so thick as one's fist, his health was bad and his blood fizy.

ALTHO' I have prescribed soap in several other cases without the same success, yet as many glandular swellings are altogether incurable, soap, if it should be found to answer in two cases out of ten, ought to be esteemed a valuable medicine.

IF it shall be objected to the virtues of soap as are solvent, that scirrhus tumours, when cut out of the body, are not dissolved by being immersed in a solution of it in water; I answer, that soap, in dissolving urinary concretions, acts like other chymical *menstrua*; but in resolving obstructed glands, it must be assisted by the motion communicated to the fluids by the heart and arteries, which it may probably stimulate into stronger contractions, and thus, as well by its resolving quality, contribute to the cure. But further, I do not imagine that soap will ever dissolve a true *scirrhus* either in the body or out of it; I only expect that it will sometimes remove glandular obstructions that are less confirmed, and of a softer kind.

QUICKSILVER and its preparations, altho' among the most powerful

erful deobstruents, if they fail of resolving hard swellings, are apt to irritate and inflame them. This effect has been generally ascribed to the weight of the mercurial medicines, but without sufficient reason; for when ten grains of calomel have raised and kept up a salivation in some persons, for two or three weeks together, and when a slight spitting has been occasioned by a grain and a half of corrosive sublimate, dissolved in spirits, and taken in four days, it is plain that the addition of weight to the mass of blood in these cases must have been so small, that no sensible change in the circulation could have been produced by it.

SOAP has not only this advantage over mercury, that it may be used in most cases, without irritating and inflaming, and consequently without any hazard of changing a scirrhus swelling into a carcinomatous one, but it does not melt down the fluids and reduce the strength like mercurials.

SOAP seems to act chiefly by its detergent quality, and perhaps, in some cases, as a true dissolvent. Every one knows the property of a solution of soap for cleaning the skin: And if a patient swallows an ounce of soap daily, his blood will in time become so saponaceous, that whilst it circulates thro' the halt-obstructed vessels of a swelled gland, it may insensibly clear away, and carry along with it that viscid matter, which, by adhering to the inside of these vessels, in a great measure filled up their cavities.

OF late the extract of the *cicuta* has been much extolled as a deobstruent*; but altho' I have tried it, as well as the powder of hemlock, in several hard swellings, some of which were external, and others situated within the *abdomen*, I have only seen it do service in two cases, one of which was a large scirrhus swelling in the left breast, and the other a hardened gland in the neck. The latter was removed by the extract of the *cicuta* in eight months; and the former, by the continued use either of this medicine, or of the powder of hemlock, has not only been kept from increasing for these four years past, but is now reduced to one third of the bulk it once had.

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* See Dr Storck's three Treatises on the virtues of the *cicuta*.

(e) IN attempting the cure of obstructions in the *viscera*, besides some of the remedies above mentioned, it will be proper to order a diet consisting of the least flatulent vegetables, weak broths, and the lighter meats. Ripe fruits, if they do not offend the stomach and bowels by their flatulence, may be useful on account of their saponaceous or resolving quality; as also goat or cow-whey, especially in the beginning of summer, when it is most impregnated with the virtues of the grass and other herbs. The patient's drink ought to be rather tepid than cold, and the best is either water alone, or mixed with a little Rhenish, or some other light white wine.

(f) EXERCISE, especially riding, is exceeding useful, not only to prevent, but to remove beginning obstructions. And here it may be proper to observe, that as those who lead a sedentary life, especially the studious, (who in reading and writing sit so much with their body bent forward,) are most subject to hypochondriac disorders and obstructions, it would be of great consequence for such, to allot some part of the day for exercise, or if that cannot be done, at least to read or write mostly standing; in which posture the abdominal *viscera* are much less compressed than in the other.

(g) IN considerable obstructions of the *viscera*, if the patient be of a full habit, the cure ought to begin with bleeding, which, by emptying the vessels, may not only tend to lessen the obstruction, but assist the action of the deobstruent medicines.

(h) WHILST by the use of some of the above remedies, we aim at a radical cure, we must not neglect to palliate the symptoms which so often attend these obstructions. This is to be done chiefly by opiates, and by the less heating carminatives and nervous medicines. But of this more hereafter.

6. VIOLENT affections of the mind.

WHEN nervous or hysteric disorders arise from this cause, the cure consists,

(a) IN avoiding all disagreeable and shocking sights, and every occasion

occasion that may be apt to excite violent passions, or commotions of the nervous system.

(b) IN strengthening the nerves, so that the mind may be less apt to be strongly affected, either by impressions from external objects, or by such ideas as arise purely from reflection; the best medicines for this purpose are the bark, bitters, steel, the cold bath, and exercise, with proper aliment; concerning all which, see the first intention of cure *.

(c) NERVOUS disorders occasioned by strong impressions of the mind, are often prevented, lessened, or cured by exciting other sensations or passions of a superior force. Of this we had a remarkable instance in the cure performed by Boerhaave, on the boys and girls in the poors-house at Haerlem †. Epileptic fits have been cured by whipping ‡. Convulsions from the toothach are removed by blisters; vomiting has been stopped by putting the hands suddenly in cold water; and a common hiccup is instantly cured by whatever excites surprize, or strongly engages the attention.

(d) NERVOUS or hysteric affections from a concealed or disappointed passion, are better cured by the fruition of the object **; or, if this cannot be obtained, by proper diet, amusements and by opiates, especially at bed-time, for composing the mind and procuring sleep, than by the whole class of nervous medicines.

H A V I N G

* Pag. 633, &c. above.

† See above, p. 581.

‡ Kaau Boerhaave Impet. faciens Hippocrat. dict. § 406.

In the Histoire de l'academie royale des sciences 1752, there is an account of a girl who was cured of epileptic fits arising from melancholy, by firing a gun at her bed-side, just as she was coming out of one of the paroxysms.

** A remarkable instance of this we have in a young man, who, from a disappointment in marriage, was suddenly seized with a *catalepsis*, so that he remained for a whole day in his chair, in the same posture, without the least motion or seeming attention to any thing about him: Nay, his whole body became as stiff as if he had been frozen. However, no sooner was he told with a loud voice, that he was to have his beloved object, than waking as out of a deep sleep, he sprung up from his seat and recovered at once. See Tuplii Observationes medicæ, lib. 1. observ. 22.

HAVING thus far treated of the cure of the several causes of nervous, hypochondriac, and hysteric disorders, I shall conclude these observations with mentioning particularly the remedies most proper for removing or palliating some of their most troublesome symptoms.

CHAP.

C H A P. VIII.

Of the cure of some of the most remarkable NERVOUS, HYPOCHONDRIAC, and HYSTERIC Symptoms.

I. **C**ONVULSIVE motions, or fixed spasms of the muscles.

THESE are either general, affecting almost the whole body, or confined to one or a few muscles or organs. As they often arise from very different causes, their radical or prophylactic cure must consist in the removal or prevention of those causes *. But as the immediate cause is, in every case, the same, *viz.* something that irritates the brain or nerves, or affects them with a disagreeable sensation, their palliative or temporary cure will be best affected by,

(1.) SUCH medicines as by their stupifying or narcotic quality lessen the sensibility of the brain and nervous system †.

IN continued spasms, as well as alternate convulsive contractions of the stomach and intestines, nothing gives so sudden or so sensible relief as opiates; which are often not less efficacious when the other muscles are spasmodically affected. In that species of the *tetanus* called *opisthotonus*, as well as that which is confined to the lower jaw, *opium* is the principal remedy ‡; and as the *hydrophobia* is only a violent convulsion of the gullet and stomach, &c. arising from

* In so far as they may proceed from some peccant matter in the blood, from phlegm, acrid humours, worms, or wind in the stomach and intestines, from a great loss of blood, an obstruction of the *menfes*, or affections of the mind, their radical cure is to be found in the preceding chapter.

† See above, p. 643, &c.

‡ See Medical inquiries and observations, vol. 1.

from the disagreeable sensation excited by any liquid touching the *fauces*, or by the effort the patient makes to swallow them, *opium* in large doses, especially if given by way of a clyster, and repeated at proper intervals, would probably be found, at least, the best palliative*.

CONVULSIVE fits of the slighter kind, which returned daily at a certain time, have been prevented by giving *opium* an hour or more before that time. But in an epileptic patient, who was seized every day about two in the morning, the fits were neither prevented nor sensibly lessened by forty or fifty drops of *laudanum*, which I ordered him to take, for several nights, about two hours before the return of the fit.

It is to be observed, that in curing or palliating violent spasms or convulsive motions, opiates must be given in larger doses than usual, and repeated more frequently; for here, as in cases of acute pain, the patients bear these medicines much better than in health.

IN some cases, especially where the vessels are full, bleeding, and other evacuations ought to precede the free use of *opium*.

(2.) SUCH medicines as, tho' not endued with a narcotic quality, are found by experience to be useful in spasms and alternate convulsions of the muscles, and seem to produce their good effects, by that *stimulus* which they communicate to the nerves, especially of the stomach and intestines. Of this kind are camphire, *castor*, musk, *asa fœtida*, the *spiritus æthereus*, spirit of hartshorn, † &c.

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* Altho' this reasoning seems to be much confirmed by the cure of Dr Nugent's patient, (See his Essay on the *hydrophobia*); yet, in this case, as musk and other remedies were used as well as opiates, it may be doubted whether the former had not some considerable share in the cure. It were to be wished, therefore, that such as shall have opportunities would make a fair trial of *opium* alone in the *hydrophobia*.

About the 20th of August 1761, a farmer's servant near Norham in Northumberland, three weeks after having been bit by a mad dog, became delirious, had violent spasms, a dread of water, and other symptoms common in such cases. He was treated by Mr Dawson surgeon in Coldstream according to Dr Nugent's method, and recovered so quickly as to be able to be employed in reaping the corn before the middle of September.

He was bled, took every three hours musk and cinnabar with honey in a bolus, and a pill of *opium*. A plaister of *galbanum* with half an ounce of *opium* was applied to the throat and neck.

† See above, p. 646.

A dram of brandy, by stimulating the nerves of the stomach, will almost instantly lessen a tremor of the hands, and in some cases make the pulse slower: And do not other stimulating medicines, in some such way, remove a palpitation of the heart, and other convulsive motions, as well as fixed spasms of the muscles? These effects may happen equally, whether those medicines excite, in the nerves, an agreeable, or an unpleasant or painful sensation. Thus a glass of warm wine with cinnamon and nutmeg, and a mixture with *aqua pulegii* or *rute*, tincture of *castor* and *asa fœtida*, will often have similar effects in flatulent and spasmodic affections of the alimentary canal; and we shall see below, that blisters and other painful applications are sometimes useful in removing spasms and convulsive motions.

(3.) SUCH remedies as relax, and at the same time, affect with an agreeable sensation the muscular fibres and nerves, rendering them thereby less liable to suffer from irritation; viz. the warm bath, *semicupium* and *pediluvium*, emollient clysters and warm fomentations applied to the feet and legs, or other parts of the body *. To this class also, we may perhaps refer venæsection, which by emptying the vessels, produces a general relaxation; but whatever be in this, we know from certain experience that it has often very sudden and remarkable effects in lessening or removing spasms and convulsive motions.

OLAUS BORICHIUS mentions a young woman liable to a periodic hiccup, which returned regularly about the same time once a-year, who, after other remedies had proved ineffectual, was always cured by bleeding largely at the arm. It was observable, that this hiccup was not lessened by the *menfes* flowing plentifully during the time the patient was affected with it †.

THE warm bath affects the nerves with an agreeable sensation, removes spasms in the small vessels, promotes an equable circulation, gently expands the fluids, and consequently fills the whole vessels of the body. But in whatever manner the warm bath and fomentations may act, their power in giving often immediate relief from

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violent

* See above, p. 645.

† Acta Hafniensia, 1671, and 1672, § 73.

violent pain, and preventing or allaying spasms and convulsive motions, has been sufficiently ascertained by experience. However, as the use of the warm *pediluvium* and fomentations applied to the feet and legs in fevers attended with a *delirium*, tremors, and convulsions *, may not be so generally known, I shall give some instances of their good effects.

(a) MRS ——— aged 23, on Saturday the third day after being delivered of her first child, was seized with a coldness and shivering, succeeded by a hot fit and sweating. Next day she was better, but after a restless night between Sunday and Monday, her skin felt hot and dry, and her pulse was quicker. Her urine which, before, had been of a natural colour, was now limpid and in too great quantity. On Tuesday her pulse was much quicker than ever, viz. at 136 strokes in a minute, but not full: She became apprehensive of dying, and, after some fits of uneasy breathing, fell into a kind of hysteric fainting, which did not affect the pulse, altho' her breathing was scarce perceptible. During this fit, which lasted about an hour and a quarter, she sighed and moaned frequently. About mid-night, she was very restless, her arms and head were convulsed, and she became quite delirious. This day a rash which had come out on Monday had in a great measure disappeared. All this time the *lochia* continued, tho' in a smaller quantity than usual. Hitherto diluents, diaphoretics, clysters, small doses of camphire, *laudanum*, blisters to the legs and sinapisms to the soles had had no effect. On Wednesday, the *delirium* increased: at noon two leeches were applied to each temple, and soon after, becoming tolerably sensible, she said she found herself much easier: But, about eight in the evening, she grew more delirious than ever, crying out in a distracted manner, and was so strongly convulsed, that with difficulty she could be kept in her bed by two strong persons employed for that purpose. At this time, I ordered
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* The success which fomenting the legs had in a case of this kind, was communicated to me about seven years since by my learned and ingenious friend Dr John Pringle physician to her Majesty. The Doctor mixed $\frac{1}{2}$ part of vinegar with the water; whereas I have always used hot water alone.

large pieces of flannel wrung out of hot water to be wrapped round her feet and legs. This application, which was renewed once in fifteen or twenty minutes, and continued near three hours, had a most happy effect; for her *delirium* and struggling to get up soon began to abate; she fell asleep at eleven, and did not awake till two in the morning, when she was quite calm and sensible; after this she slept near three hours more, and was pretty easy and clear in her head all Thursday, till the evening, when the *delirium* returned in a less degree. But this symptom being soon removed by fresh fomentations, she passed a good night, and gradually recovered, without any return of the raving, fainting, or convulsions.

(b) R. B. a boy aged eleven, was seized with a headach and fever. Monday, he complained of a sharp pain in his right side, on which account near eight ounces of blood were taken from him. On Tuesday, his head was easier, but the pain in the side continued; his pulse beat 120 times in a minute. This evening, a blister was applied to the right side. Next morning, the pain was much abated, but a slight *delirium* began, and increased towards the evening, when his pulse was about 130, tho' no-ways full. Leeches were applied to the temples, and poultices to the feet. On Thursday morning, the *delirium* and fever continuing, his head was shaved, and afterwards fomented with cloths wrung out of hot water. This made him somewhat drowsy and calmer for a short time; but about mid-day, his pulse became quicker, sharper, and smaller, and the *delirium* increased. At three after noon he was quite insensible, had a *subfultus tendinum* with catchings, and his pulse, which was small, beat near 150 times in a minute. In this state I ordered his feet and legs to be immediately fomented, as in the preceding case: the effect was, that he fell soon asleep and waked at four o'clock, somewhat calmer, and with a little sweat on his skin. The fomentations were renewed; he slept again, and about six in the evening he was much less delirious, and his pulse did not exceed 124. At eight o'clock, the legs were fomented again, for a considerable time. He had several good naps during the night, and Friday morning,

was perfectly sensible, with a pulse at 96. From this time he recovered daily, without any return of the fever or *delirium*.

(c) A middle-aged man, who was seized with a continued fever, in a few days became delirious, had a quick and very small pulse, a parched tongue, flushings in his face, and twitchings; and he passed his urine insensibly. He was treated with the common remedies, and had a blister applied between his shoulders, but with little advantage. My advice being asked about the 8th or 9th day of the disease, I ordered his legs and feet to be fomented with flannel wrung out of hot water. This, in a little time, put him asleep; and, next day, his pulse was fuller and less quick, the *delirium* was abated, his tongue was moister, and a gentle sweat came on. After this, the fever decreased gradually, and the patient recovered.

HAVING found such benefit from the warm fomentations in fevers attended with a *delirium*, tremors, and spasms, I thought it might be worth while to make trial also of the warm *pediluvium* in such cases; and I soon found this to have the same, but more powerful effects than the fomentations: for, in some cases, when these last had failed to lessen the tossing, raving, and convulsions, the *pediluvium* succeeded, not only in the time the patient used it, but its effects continued a good while after; and when they ceased, it was renewed again with the same advantage as before. In some cases, I have ordered the feet and legs to be put in warm water four or five times in 24 hours, and to continue in it from half an hour to near a hour at a time, unless the patients became faintish.

(a) A lady about 20, on the fourth day after being delivered of her first child, began to be feverish, and could not sleep. After this she became very delirious, talked constantly, had sometimes tremors, and was so restless, that for two days she had not lain one minute in the same posture, and was with difficulty kept in bed by two or three nurses. On the 10th day after her delivery, when I first saw her, the symptoms now mentioned were all increased; she was now quite silent, and seemed to understand nothing that was said to her; her pulse, which was but of a moderate strength, beat above 150 times in a minute. Nay once, when she was more agitated.

tated than usual, it rose to 180 strokes in that time, and became withal very small. As she had been blooded and blistered, and used several other remedies without advantage, I ordered her feet and legs to be put immediately into the warm *pediluvium*, which was done by making her sit up on the bed-side. At first, it required two people to keep her feet in the water, but, in less than a quarter of an hour, she grew calmer, and made little motion either with her legs or any other part of her body. After using the *pediluvium* for half an hour, she was put to bed, but soon began to grow as restless as formerly; upon which account warm fomentations were applied to her legs and feet, and renewed from time to time for near two hours, but without any benefit. I therefore thought it best to renew the *pediluvium*, which was used at this time for a full hour; it soon made her sit quiet, and after she was put to bed, altho' she did not fall asleep, yet she lay several hours without tossing as usual, and her pulse was reduced to 136. As often as she began to be any ways restless, the *pediluvium* was renewed. After using it the fourth time, she got several short naps, was less delirious, and her pulse only made 120 strokes in a minute; from this time (*viz.* the eleventh day after her delivery) the *pediluvium*, which was never repeated above twice in 24 hours, procured her better and less interrupted rest, and lessened all her bad symptoms; so that in two days more she was quite free from the *delirium*, and her pulse did not exceed 90 in a minute.

(b) A gentleman aged 40, after having had a continued fever for 7 or 8 days, began to rave, and the *delirium* increased so much, that about the 11th day, he could not be kept in bed; nor would he allow either fomentations, blisters, or sinapisms to be applied to his legs or feet. In this condition I advised to take him out of bed, as often as he insisted upon rising, and while he sat up, to keep his feet and legs in warm water. Between seven and half an hour past eleven in the evening, he was seven times out of bed, and as often used the *pediluvium* for about half a quarter of an hour, or longer, at a time. Before midnight the hurry of his spirits began to abate, he talked less, and seemed drowsy. In the first part of the

the night he slept a little, and towards the morning he slept three hours at once. His pulse was now reduced from 120 to 100 strokes in a minute; and from this time the *delirium* decreased gradually for several days, and he recovered.

(c) HAVING been called to A. A. aged 30, in a continued fever with inflamed eyes, and so violent a *delirium*, that he could not be kept in bed; I ordered him to be taken up, and to have his feet and legs put in warm water for twenty minutes. This was done thrice in the space of thirteen hours; and it always lessened his raving, made him quieter, and procured sleep after he went to bed. Next day, he became a good deal comatous, and his eyes were rather more inflamed; but the day after, he grew more sensible, his eyes looked better, and his pulse had fallen from about 170, (which was its quickness when at the worst,) to 128 strokes in a minute. After this, he recovered gradually.

(d) ANOTHER patient, aged 25, in a continued fever, with a pulse above 140, inflamed eyes, and a violent *delirium*, so that two strong men had been employed to keep him in bed, recovered after being taken up thrice, and having his feet and legs kept in warm water for above twenty minutes each time. The warm water always lessened the *delirium*, and, after he returned to bed, made him to fall asleep.

INSTEAD of adding more cases, I shall only observe, that I have saved more patients who appeared to be in great danger, in the delirious state of a fever, by the fomentations, and especially by the warm *pediluvium*, than by any other remedy: and even in those cases, where these applications were insufficient to compleat the cure, they, almost always, gave some present relief, by making the patients somewhat quieter and disposing them to sleep.

THE fomentations and warm bath to the feet are particularly useful in fevers, where the brain and nervous system are much irritated. In cases where the eyes are inflamed, they will answer better, if the patient has been bled at the temples with leeches, before their application. When the sick cannot bear the *pediluvium* in an erect posture, I order their legs to be put over the side of
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the bed, so as they may be immersed in the warm water; the heat of which should not be less than 100 degrees of Fahrenheit's scale.

I shall only add on this subject, that I have found the warm *semicupium*, or *pediluvium*, the best remedy for those convulsions which sometimes precede the eruption of the small pox; and for that general tremor of the whole body which often happens towards the end of that disease, when the pustules are of a very bad kind. But to return,

(4.) IN convulsive motions or spasms, such remedies are often useful, as by painfully affecting the nerves of some part of the body that is sound, in a great measure lessen or destroy the sense of that irritation which was the cause of those symptoms*. Of this kind are blisters, acrid cataplasms, dry cupping, friction, and the cold bath.

ABOUT seventeen years since, a woman, aged 20, was seized with an alternate motion of the abdominal muscles. In the night, when in bed, she was pretty free from this ailment; but, through the day, those muscles were almost constantly in motion, and it was not in her power to restrain them in the smallest degree. After she had tried many medicines, without any benefit, I ordered a circular blister, of about eight inches diameter, to be applied to the *abdomen*. This put a stop to the convulsive motions for several days; and although they returned afterwards, they were much weaker and less frequent, and in a short time, they ceased entirely, without the assistance of any medicine, except a few doses of camphire.

IN cases, where epileptic convulsions took their rise from an uneasy sensation in some part of the arm or leg, I have found blisters applied to these parts the best remedy. It may be proper, however, to observe, that, in people whose nerves are uncommonly delicate and sensible, it is often better to attempt the cure of convulsive motions or spasms by opiates, musk, camphire, and the warm bath or *pediluvium*, than by blisters, which sometimes do harm by the violent pain which they occasion.

I have found dry cupping not only useful in convulsive contractions

* Hippocrat. Aphor. lib. 2. No. 46.

tractions of muscles *, but also in removing recent rheumatic pains from cold, where there was no fever; nay, in older pains of this kind, I have seen patients relieved, at least for some time, by this remedy. I order the cupping glass to be applied to the pained part and all round it, and let it stick each time three or four minutes, or till it falls off. The suction is often so strong as to occasion small effusions of blood below the scarf-skin. The good effects of dry cupping do not proceed solely from the pain it occasions, which is not very considerable; but chiefly from the change it makes in the circulation of the blood through the subcutaneous parts: for, while the cupping glass remains fixed, the blood which used to be sent to the parts below, is, in a good measure, derived into the vessels of the *membrana adiposa* and skin; and, even for some time after, the motion of fluids through these parts continues to be greater than usual, on account of that irritation and slight degree of inflammation which is generally occasioned by cupping.

THE cold bath is often useful in curing those convulsions which go by the name of *St Vitus's dance*: And cold water thrown on a person labouring under the *hydrophobia* has enabled him, for some time, to drink pretty freely †. Was not this effect owing to the strong impression made on the nervous system by the cold water, which, in some measure, destroyed or lessened the unnatural sensibility of the nerves of the *fauces* and gullet? For the inability to swallow liquids in the *hydrophobia* is not owing to a palsy of the throat, as some authors of great character have thought, but solely to the disagreeable sensation excited in the *fauces* and gullet, by the touch of water and other fluids, which raise as great spasms and

* A man, aged about 50, who had for many years been constantly afflicted with an alternate motion of the muscles of his head and neck, found more benefit from dry cupping along the back part of the neck and shoulders, than from any other remedy. It is true indeed, the good effects of this application lasted only for a few days; but, had the disorder been less fixed, it is probable, that repeated cupping might have made a perfect cure.

† See Mead on poisons, edit. 5. pag. 182. and Van Swieten. Comment. in Aphor. Boerhaave, tom. 3. p. 576.

and convulsive contractions in those parts and the stomach, as they do in the muscles of respiration, when, by an accident in swallowing, they get into the *trachea*.

(5.) FEAR, surprise, attention, or other strong affections of the mind, will frequently put a stop to convulsive motions and spasms, and sometimes succeed after other remedies have failed, as in the following case.

A girl aged eight, in the beginning of September 1759, was seized with an alternate motion of the *masseter* and temporal muscles, for which no cause could be assigned. This motion exactly imitated the pulsation of the heart. Only those muscles were contracted and relaxed above 140 times in a minute, while the heart did not make above 90 strokes. Their contractions were all of equal strength, and the intervals between them were also equal. When the patient pressed the teeth of the lower jaw strongly against those of the upper one, by a voluntary contraction of the *masseter* and temporal muscles, their convulsive motions were much less remarkable; and when she pulled down the lower jaw as much as she could, and by the continued action of its muscles, kept it in this situation, the *masseter* and temporal muscles were no ways convulsed. Before I saw this patient, she had been blistered upon the course of the affected muscles, which lessened their convulsive motions, while the blistered parts continued to run, but no longer. I ordered plaisters of the *emplastrum antihystericum* with some *opium* to be applied where the blisters had formerly been. These were kept on no longer than two days, during which time the convulsions were weaker and less frequent, not being repeated above 50 or 60 times in a minute; however, in a day or two after the removal of these plaisters, the convulsive contractions became as strong and as frequent as ever. Brimstone, in powder, was rubbed on the temples and cheeks without any visible effect. Suspecting that this convulsive disorder might, perhaps, proceed from worms, I prescribed a bolus of rhubarb with calomel, which the girl obstinately refusing to take, her father went to fetch a horse-whip to beat her. The fear of this affected her so strongly, that, without

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the bolus, the convulsions of the *masseter* and temporal muscles instantly ceased; and have never returned since, except once on occasion of a fright, when they continued near an hour, and then went off without any remedy.

CELSUS, in the *spasmus cynicus*, recommends pouring on the patient's head warm sea-water and sulphur *: And a roll of brimstone held in the hand is frequently used now-a-days as a cure for cramps or fixed spasms of the muscles; and I have known it succeed in several cases. The snapping of the brimstone, which often happens, has been by some ascribed to the electrical fire being discharged in great quantity out of the body; but without any reason. The truth is, that a roll of pure brimstone held in the hand when warm, will frequently break, whether the person be affected with the cramp or not; and the same thing happens to brimstone, when placed before the fire in a heat equal to, or a little greater than that of the human body. I am therefore of opinion, that brimstone cures spasms not by any medical virtue; but that its effects are to be ascribed to the patient's attention † and faith, or rather to the surprize occasioned by the roll snapping in his hand: And as a confirmation of this, I have known some affected with the cramp, who, having been informed that the breaking of the brimstone was owing to the heat of the hand, missed of a cure.

(6.) CONVULSIVE motions or spasms are often prevented or cured by compression, which braces and renders firmer such parts of the body as are most subject to them. Thus cramps in the legs are prevented by tight bandages; and when convulsions arise from a flatulent distension of the intestines, or from spasms beginning in them, they may be often lessened or cured by making a pretty strong compression upon the *abdomen* by means of a broad belt. The Baron Van Swieten mentions the case of a young lady, whose legs, thighs, and belly, were kept tight with rollers for several months in order to prevent convulsions, which, from an uncommon

* De medicina, lib. 4. cap. 2.

† I have been often cured of a slight hiccup by looking stedfastly, for two or three minutes, on the impression upon a shilling, or any other coin: And I know a lady who, tho' very liable to hysteric fits, is never affected with them, or even slighter complaints, when any of her children happen to be dangerously ill.

mon delicacy of her nerves she was frequently subject to *. Epileptic fits, which take their rise from a peculiar sensation in some part of the legs or arms, may be kept off by making a tight ligature about these members as soon as that sensation begins, or at least before it has reached the superior parts of the body.

To the remedies already mentioned may be added the bark, which has sometimes cured periodic convulsions after other medicines had failed †.

I shall only observe further, that when spasms, or convulsive motions arise from sharp humours in the stomach and intestines, nothing will procure any lasting relief till these are either corrected ‡ or expelled ||.

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II. HYTE-

* Comment. in Aphor. Boerhaave, tom. 1.

† See Philosoph. transact. No. 174.

‡ A young man under 20 years of age, in a continued fever, was affected with a strong *delirium* and convulsions of his face, throat, and almost all the parts of his body, particularly his arms and legs. At the same time, he complained of a great internal heat and thirst. After having been in this way for two or three days, he had a sweet orange given him, which he eat greedily, and, calling always for more, consumed near two dozen of them in two days. After he began to eat the oranges, the convulsions abated, and went quite off in three days.

|| For the following case, in which violent convulsive fits were removed by repeated vomits, I am obliged to Dr John Gardiner, physician in this place.

A young woman of 17 years of age, of a delicate constitution, after having been a good deal fatigued, was seized, on the 20th of July, with convulsions of almost every part of her body, which continued about five minutes; after which she fainted away, and the convulsions ceased; but upon her recovery, the convulsive motions of her arms, and the muscles of respiration, returned. These convulsions having continued, except in time of sleep, to the 22d, when I was called, I ordered a bolus of ten grains of musk to be taken every three or four hours, with two table-spoonfuls of a musk julep. On the 22d, 23d, and 24th, she was several times free from the convulsions for half an hour, or sometimes an hour: But, upon the least noise in the room, or any thing that occasioned surprize, they returned. The drawing the curtain of her bed, or the lifting the latch of the door, used to have this effect. Nay, although she saw her sisters going to open or shut the door, or to handle the tea-cups, and therefore expected to hear some little noise; yet so powerful was the impression made by it on her nerves, that by no effort could she prevent the convulsions from coming on.

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II. HYSTERIC faintings with convulsions.

If the pulse be full, or the patient any ways plethoric, some blood should be taken away; after which, we may endeavour to rouse her by the smoke of *asa fetida* or burnt feathers, or by
oleum.

In order to lessen this uncommon sensibility of the nervous system, camphire was added to the musk-bolus, and she took 12 drops of *laudanum* in a dish of valerian tea five or six times in 24 hours. After this, the intervals between the convulsions became longer; but when they returned, they were more severe. Although her *menfes* came on the 27th at the usual time, her symptoms did not abate. After this flux ceased, she had a blister applied between her shoulders, she used a solution of *asa fetida*, and had the *laudanum* increased to a hundred drops a-day.

On the 31st of July, she began to be seized with faintings for about five minutes at a time; and soon after this, the convulsions became universal, and attacked her from twelve to eighteen times a-day in regular paroxysms, which lasted two or three minutes. In the night, she was almost always free from them. I then prescribed for her an ounce of the bark, half an ounce of valerian, and a drachm of castor, to be made with syrup of white poppies into an electuary, of which she took the size of a nutmeg three or four times a-day; at the same time, the *laudanum* and boluses of musk were continued. After the 3d of August, she was attacked with severe asthmatic fits, which, together with her faintings and convulsions, often made the number of paroxysms amount to thirty in a day. Some days after this, upon the convulsions leaving her, she was seized with continued spasms in her arms, legs, and thighs; after which she sometimes complained of a small degree of pain and confusion in her head.

Her pulse during all these complaints seldom exceeded 80 strokes in a minute, nor were there any appearances of her stomach being disordered. However, on the 9th of August, I prescribed a vomit of *ipecacuanha*, which made her throw up a great deal of dark greenish and very bitter bile; about an hour after this, she was attacked with one of the convulsive fits, but had no more of them that day. On the 10th of August, she had twelve, and on the 11th, fourteen of these fits. Upon the 12th, in the morning, she took another vomit, which was also repeated on the 13th. Each time she threw up a good deal of bile, and had no fit on either of these two days. On the 14th, she took a decoction of tamarinds with *fenna*, which purged her five or six times, and in the evening she was attacked six times with the convulsions and fainting fits. On the 15th their number was almost double to that of the preceeding day; but on the 16th, when the vomit was repeated, she escaped them altogether. Every other day for a week, she took a vomit of *pulv. ipecacuanhae gr. v.* and *tart. emet. gr. i.* and at night, sometimes, a small dose of *elixir sacrum*; by which means, before the beginning of September, she got quite free from the fainting fits and convulsions.

oleum succini and spirit of hartshorn dropt on cotton, and put into the nostrils. These medicines, by the strong and sudden impression they make on the very sensible nerves of the nose, not only tend to excite the several organs into action, but to lessen or destroy the disagreeable sensation in that part of the body which brought on the fit. With the same view hot bricks may be applied to the soles of the feet; and the legs, arms, and belly may be strongly rubbed. But there is no remedy which I have found so effectual in removing hysteric faintings with convulsions, as the warm *pediluvium*; for, after many other things had been tried to no purpose, I have seen the patients restored to their senses, almost instantly, by putting their feet and legs in water a little more than blood-warm: And it was remarkable, that upon discontinuing the *pediluvium* too soon, the fainting and catchings often returned in a less degree, and the pulse became smaller and irregular. In a few cases, where the patients were plethoric, and the convulsions very strong, the *pediluvium* has failed.

WARM water thus used, is not only the speediest, but the safest cure for hysteric faintings; while strong volatile spirits held to the nose are apt to throw some very delicate women into more violent convulsions.

IN case of costiveness, a laxative clyster with *asa fetida* will be proper; and, as soon as the patient can swallow, two table-spoonfuls of a solution of *asa fetida*, or some cordial julep may be given.

AFTER the fit is over, the radical cure must vary according to the different causes from which it may proceed. However, such medicines will commonly be found most efficacious as strengthen the alimentary canal and the whole nervous system. An antihysteric plaister applied to the *abdomen* has been, in some cases, useful; as also gentle vomits and stomachic purges.

III. A violent pain with cramps in the stomach.

THE method which I have found most successful in this case, is
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It was observable, that, during her illness, in the intervals of the fits, she was often very cheerful, and sometimes jocular; but after she recovered, she became grave, thoughtful, and somewhat morose, which was her natural disposition.

to make the patient, if there be any inclination to vomit, take some draughts of warm water to clean his stomach. After this, I order a clyster of six ounces of water, and from 50 to 80 drops of *laudanum*. This is much surer than *laudanum* given by the mouth, which is often vomited up; and, in some cases, increases the pain and spasms in the stomach.

If the pain and cramps return with great violence, after the effects of the anodyne clyster are over, I order another to be given, with an equal or larger quantity of *laudanum*; and, once in four hours, two table-spoonfuls of such a julep as the following.

R. Mosch. scrup. ii.
 optime teratur cum
 Sacchar. alb. drach. ii.
 Dein adde
 Mucilag. gum. Arab. unc. fs.
 Aqu. cinnamom. f. v.
 menth. piper. ana unc. ii.
 aromat. drach. vi.
 M. f. a.

If the patient has been costive, a laxative clyster must be given before the anodyne ones.

THE anodyne balsam rubbed into the stomach, and the warm *semicupium* are often useful. After the pain and cramps have been removed, the *emplastrum antihystericum* applied to the epigastric region has sometimes contributed to prevent their return.

IN all very violent or lasting pains of the stomach, some blood ought to be taken away, unless the weakness of the patient makes it improper; for this evacuation will always lessen the danger of an inflammation, and can seldom do any considerable harm.

WHEN the pain or spasms in the stomach proceed from a suppression of the *menfes*, venæsection is of great use. If they are owing to the true gout, besides *laudanum* and musk, spices, and some of the stronger cordial waters, or a large dram of brandy or rum, will be necessary, together with blisters to the ancles.

IV. AN

IV. AN indigestion and vomiting, with pains in the stomach.

1. WHEN these complaints proceed from noxious humours in the stomach, the best remedies are vomits and gentle stomachic purges; together with elixir of vitriol, or the testaceous powders, according to the different nature of those humours.

2. WHEN, from scirrhus obstructions in the alimentary canal, we can do little more than to palliate by means of grateful stomachic medicines, and opiates. However, in cases of this kind, a small glass of Spa or Pyrmont water frequently repeated, has sometimes staid on the stomach, when every thing else has been thrown up.

WHEN there is a scirrhus obstruction in the coats of the stomach near the *pylorus*, this passage is often so much straitened, that only the thinner part of the aliment can get into the *duodenum*; while the more solid part, after remaining several hours in the stomach, and occasioning heart-burning and sickness, is at last discharged by vomiting. Patients in this situation always find themselves easiest when they use only the thinner kinds of aliments, such as light broths, milk, panada, sago, salep, and the like.

3. WHEN a sickness and pain in the stomach, with vomiting soon after eating, are owing to a too great delicacy, or an unnatural sensibility of the nerves of the stomach, either in consequence of an irregularity of the *menstrua*, or of some acrid humour in the blood falling on those nerves; while we palliate with agreeable cordials and aromatics, we must endeavour to strengthen the stomach by the bark, bitters, chalybeates, and exercise. But, in cases of this kind, I have found nothing produce such immediate good effects as *laudanum* given an hour or more before dinner or supper.

(a) AN unmarried gentlewoman aged 44, irregular as to the *menfes*, was seized with a pain in her stomach, and soon after every meal became sick, and vomited what she had eat. After having been in this way for eight or ten days, she took a vomit of *ipecacuanha*, several doses of the *elixir sacrum*, and *tinctura rhabarbari amara*;

ra ; she also used warm claret with cinnamon and nutmeg, and a julep of pepper-mint water with the *spirit. volat. oleos.* but without any advantage. As she slept ill, I advised her to take twenty drops of *laudanum* at bed-time, which made her rest better in the night, but did not lessen the vomiting the following day. Next night I desired her to take the *laudanum*, not at bed-time, but an hour before supper. The first dose, in this way, prevented her vomiting after supper, and next day after breakfast ; but she threw up her dinner as usual. However, by increasing the *laudanum*, before supper, to twenty-five drops, in three or four days, she got free of the pain and sickness at her stomach, as well as of the vomiting after meals.

(b) A married lady, aged about 30, after having been, for some time, irregular as to the monthly evacuation, upon eating freely of almost any kind of meat, but especially such as lay heavy on her stomach, was apt to be affected with sickness, faintings, and slight convulsive motions, attended with a small irregular and quick pulse, and a coldness of her whole body. After she had used vomits, the bark, bitters, sacred elixir, and various grateful stomachic medicines to little purpose, I advised her to take some *laudanum* every day, an hour or two before dinner. Having been formerly much accustomed to this medicine, she began with thirty-five drops, and soon increased them to fifty or sixty. The *laudanum*, instead of making her drowsy, gave her better spirits, and enabled her to eat at dinner as usual, without being affected with sickness or faintings after it. She continued the use of the *laudanum* in this way, pretty constantly, for five or six weeks. Some days when she had neglected the *laudanum* before dinner, if she took it as soon as she began to be uneasy after eating, it soon lessened the sickness at her stomach, and prevented the faintings and convulsive motions.

(c) ANOTHER married lady, aged about thirty, who had been often troubled with a pain, a sourness and wind in her stomach, and when these left her, with asthmatic fits, complained of a lump in her throat, flatulence, and such a weakness of her stomach and indigestion,

indigestion, that every kind of food occasioned pain, sickness, and vomiting, except bread and wine, or a very little boiled or roasted chicken. After trying the bark, vomits, sacred elixir, and exercise, with scarce any benefit, I desired her to take some *laudanum* an hour before dinner. Altho' she did not exceed sixteen drops, yet she always eat her dinner better, and digested it with the same ease, as when she was in health; nor did she find any inconvenience from the *laudanum*, except that it made her thirsty in the afternoon.

4. IN some cases I have known a pain in the stomach with vomiting after eating, cured by soap taken daily to the quantity of two drachms; in other cases half a pint of tepid lime-water, drunk thrice a-day, has answered better than the soap.

WHEN a heat and soreness in the stomach arise from an acid, the testaceous powders ought to be taken freely. They have also cured, at least for the time, some who, upon drinking a glass of wine, have felt in their stomach a burning heat instantly spreading thro' almost their whole body.

SEVERAL persons have found great relief from a pain in the stomach, both before and after eating, by taking a large draught of warm water, with a little wine or brandy in it.

IN the second volume of the Medical Inquiries and Observations, we have an account of a violent pain in the region of the right kidney in one patient, and of a pain in the stomach in another, immediately relieved by a draught of equal parts of fountain-water boiling, and Pyrmont or Bath water. But I imagine the relief procured in these cases was rather owing to the warmth of the liquor than any virtue in the Pyrmont or Bath water; for a patient of mine near eighty years of age, who, after having been long subject to bloody urine, came at last to have an ulcer in his bladder, found his pains always much lessened, and sometimes almost quite removed, by drinking largely of Arabic emulsion, tea, milk and water, or weak broth, a good deal more than blood-warm. As the good effect of these warm liquors was always immediate, it must have been owing solely to their action on the nerves of the

stomach. We know that warm water applied externally often eases internal pains; it is no wonder then, that warm liquors received into the stomach, a part much more sensible than the skin, and whose nerves have a remarkable sympathy with almost every part of the body, should have equal or more powerful effects in relieving pains, even in such parts as are not immediately connected with it.

V. A cholic of the hysteric or flatulent kind.

If the patient be costive, as is almost always the case, the body must be opened by laxative clysters, to which a drachm or two of *asa fetida* may be added. If there are violent vomitings, after several draughts of toast and water, a mixture ought to be given of salt of wormwood, lemon-juice, and pepper-mint water *, together with *laudanum*. However, these draughts are often ineffectual, and in some few patients the vomiting is increased by the *laudanum*. In such cases I have always succeeded, by ordering a clyster of six ounces of water, with fifty, sixty, or even eighty drops of *laudanum*; and when no thorough passage could be procured, I gave, by favour of this opiate, some pills of aloes with calomel; which passing into the intestines before the vomiting returned, generally procured a plentiful

* The draughts of salt of wormwood and juice of lemons are observed, in a great measure, to lose their power of stopping a vomiting, when they are not swallowed in the act of effervescence: And is not their superior anti-emetic power, in this state, owing to their making a much stronger impression upon the nerves of the stomach, while they continue to emit their fixed air, and when all their parts are in violent motion, than after saturation, when they can act only by their saline quality? For, while the nerves of the stomach are affected with this brisk and unusual *stimulus*, that disagreeable sensation which produced the vomiting must be lessened or destroyed. And is it not the effect, which those draughts sometimes have in preventing the attack of intermittent fevers, to be ascribed solely to their action on the very sensible nerves of the stomach, and not to any sudden change which they may be supposed to produce in the nature of the humours contained in the *primæ viæ*? Further, are not many of those mineral waters which contain a good deal of fixed air and sparkle in the glass, much more grateful, as well as invigorating to the stomach when drunk at the well, than after they have stood for some time in an open vessel, because in this last case they have, together with their fixed air, lost their power of gratefully stimulating the nerves of the stomach?

ful evacuation by stool, which either wholly or in a great measure removed the disease.

If the purging pills fail to open the body, and the pain and vomiting return, another anodyne clyster must be given, and soon after it a larger dose of the pills; and a little before the time these pills may be expected to work, the patient should go into the warm bath. In patients of a full habit, especially if the pain be very great, some blood ought to be taken away.

To prevent the frequent return of hysteric colics, an antihysteric plaister applied to the *abdomen*, a dose of the sacred tincture or elixir once a-week, and exercise, especially riding, will be found useful. A milk-diet has sometimes cured those who have been much afflicted with those colics; and the sulphureous water of Mof-fat, drunk for two or three months in the summer, has in some cases made their returns much less frequent.

VI. FLATULENCE in the stomach and bowels *.

THE medicines most proper in complaints of this kind, are either such as procure speedy relief by expelling the wind, or those which, by strengthening the alimentary canal, lessen its generation. Among the former I have found none more efficacious than the *spiritus æthereus* and *laudanum*. I commonly give the *laudanum* in a mixture with peppermint-water and tincture of *castor*, or *spiritus natri dulcis*. In some, in place of this, I prescribe *opium* in pills with *asa fetida*. And here it may be worth while to observe, that the good effects of opiates are equally conspicuous, whether the flatulence be contained in the stomach or intestines; whereas those warm medicines commonly called *carminative* do not often give immediate relief, except when the wind is in the stomach.

WITH regard to the *spiritus æthereus*, I have frequently seen very good effects from it in flatulent complaints; of which I shall content myself with giving one instance. A lady, aged between 40 and

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* I have often found great benefit, in flatulent complaints of the stomach and bowels, from frequent rubbing of the legs with a flesh brush.

50, about the time the *menfes* were leaving her, found her belly increase so much in bulk, that for some time she suspected herself to be with child. In the morning, she was often so much swelled about the stomach, as not to be able to bear her stays, or breathe freely. She used a variety of medicines; but nothing gave such immediate relief as a tea-spoonful of the *spiritus æthereus*, mixed with two table-spoonfuls of water. This always made her bring up a good deal of wind, and lessened the straitness and swelling about her stomach.

IN gouty cases, the *spiritus æthereus*, a dram of French brandy, or of the *aqua aromatica*, and ginger, either in substance, or infused in boiling water, are among the best medicines to expel wind.

WHEN the case of flatulent patients is such as to make it improper to give them warm medicines inwardly, a plaister made of equal parts of the *emplastrum antihystericum* and *stomachicum* may be applied to the stomach or belly with advantage; or four or five tea-spoonfuls of the following liniment may be well rubbed on these parts at bed-time.

R. Bals. anodyn. Batean. unc. i.

Ol. mac. per express. unc. ss.

menth. drach. ii. Misce.

THE remedies most proper for strengthening the stomach and bowels, and consequently for lessening the production of flatulence, are the bark, bitters, chalybeates, and exercise. In flatulent cases, I add to the tincture of the bark and bitters, which I have so often recommended, some nutmeg or ginger. And when I prescribe the filings of iron, I join them with the *pulvis diaromaton*. When windy complaints are attended with costiveness, nothing answers better than four or five of the following pills every other night at bed-time.

R. Af. foetid. drach. ii.

Aloes socotrin.

Sal. Mart.

Rad. zinziber. ana drach. i.

Elix. proprietat. q. f. ut. f. pil. gr. iv.

ON the other hand, when the body is too open, twelve or fifteen grains of rhubarb, with half a drachm or two scruples of the *con-*
fectio

fectio Japonica, given every other evening, will have very good effects.

IN those flatulent complaints which come on about the time the *menfes* cease, repeated small bleedings often give more relief than any other remedy.

WITH regard to diet, I shall only observe, that tea and all flatulent aliments are to be avoided ; and that for drink, water with a little brandy or rum, is not only preferable to malt-liquor, but in most cases also to wine.

VII. A nervous or spasmodic asthma.

(1) IN the true spasmodic asthma, where there is no fixed obstruction in the lungs, nor any load of phlegm oppressing them, the fits are best relieved by bleeding and *opium*. If the patient be of a full habit, we may bleed largely ; if otherwise, we must either take away little blood, or omit this evacuation altogether.

THE *opium* may be given either in the form of the *elixir paregoricum*, or in such a draught as the following.

R. Aqu. menth. unc. i. fs.
Laud. liquid.
Sp. volat. oleos. ana gutt. xxv.
Syr. commun. drach. ii. Misce.

SIR Richard Blackmore tells of a physician, much affected in the winter-time with a dry asthma, who every morning took thirty drops of *laudanum*, without which he found himself unable to go abroad about his business.

NOR are opiates less successful when a true spasmodic asthma arises from sympathy with the stomach, than when the nerves of the lungs are themselves primarily affected*.

THAT

* A gentleman, aged 25, after having taken mercury for several weeks, on account of a venereal disorder, became peevish because of his confinement, and would eat no meat for upwards of twenty-four hours, but drank largely of whey and water-gruel. In the afternoon he began to be affected with a difficulty of breathing, unattended with any cough or spitting. The tincture of *castor*, spirit of hartshorn, and other medicines, which were prescribed, gave very little relief ; and the asthmatic fit becoming much worse about midnight, a draught with twenty drops of *laudanum* was ordered. This soon lessened the difficulty of breathing so much, that he fell asleep, and next morning awaked in a great measure free from this complaint, which, after eating some broiled chicken for dinner, left him intirely.

THAT sense of faintness about the stomach, with a frequent sighing and a difficulty of breathing, with which women after child-bearing are sometimes affected, when the miliary eruption does not come properly out, are often lessened or removed by a dose of the *elixir paregoricum*, or a bolus of *castor. sal. corn. cerv.* and *opium*.

IN the true spasmodic asthma, especially when it is owing to wind in the stomach and bowels, or increased by this, a solution of *asa fetida*, the tincture of *castor* and spirit of hartshorn, are often useful, although their antispasmodic virtue is much less than that of *opium*.

(2.) WHEN an asthma of the spasmodic kind is occasioned by or attended with some fixed obstructions in the lungs, or a considerable accumulation of humours in them, we must use a method of cure somewhat different from the above. For although bleeding is equally useful, and often more necessary here, than in the true spasmodic asthma; yet opiates are not to be given to lessen the fits till after the lungs have been sufficiently cleared by evacuations and attenuating medicines. A large blister between the shoulders is of excellent use to promote expectoration and relieve the lungs. Vomits are likewise proper, but cannot be safely given till after the asthmatic fit begins to abate. In some patients a purge of *manna* and Glauber's salt, or of soluble tartar, almost always lessens or carries off the fit: while in others, who have weaker bowels, whatever purges briskly, whether it be food or medicine, is apt to bring on or at least to increase the fit.

FOR present relief, I commonly give spirit of hartshorn, or compounded tincture of *castor*, diluted with a sufficient quantity of water. With the same view also, a table-spoonful of a solution of equal parts of *gum. ammoniac* and *asa fetida* in penny-royal water, may be taken five or six times in twenty-four hours.

A draught of water, with an eighth part of vinegar, and sweetened with honey or sugar, often gives considerable relief in asthmatic fits; altho' such is the difference of constitutions, that I have met with some persons whose breathing was always made worse by acids of every kind.

THOSE

THOSE asthmatic patients, whose stomach and bowels are weak, and much troubled with flatulence, do better with the lighter flesh-meats and a little wine, than with a milk and vegetable diet; and the solution of *gum ammoniac*, with *acetum scilliticum*, or the *pilule scillitice*, do not commonly agree so well with them as the *asa fetida* and volatile alkaline salts.

WHEN elderly persons have been seized with an asthmatic paroxysm from the gout attacking the lungs, I have found most benefit from blisters applied between the shoulders, and to the legs, and from boluses of *gum ammoniac*, *sal. vol. ammon.* and camphire, given twice or thrice a-day.

(3.) To prevent the return of the fits in the true spasmodic asthma, we must endeavour to strengthen the lungs and whole nervous system by means of the bark, chalybeates, elixir of vitriol, a proper diet, country-air, and riding.

A flannel waistcoat next the skin, or a large piece of flannel wore on the breast, has contributed to prevent the frequent return of asthmatic fits.

THE patients should, above all things, avoid eating or drinking so much at once as to burden their stomach.

IN the mixt *asthma* the bark must be used with more caution, especially if the lungs be considerably obstructed, or loaded with phlegm; and the cure must be chiefly attempted by issues in the back and arms, or a seton in the side; and by other medicines that tend to remove the obstruction in the pulmonary vessels, or lessen the flux of humours to them; of this kind are the *pilule scillitice*, taken in such quantity as to keep the body always open; pills of garlic and soap; the juice of forty or fifty *millepedæ* in two or three table-spoonfuls of French white wine, Rhenish or cyder, taken twice a-day; and crude mercury, or quicksilver pills, which have sometimes cured asthmatic ailments after other remedies had failed.

As not only different patients are relieved by different remedies, but the same patients, from a change in their constitution, or in the nature of the disorder, often require a considerable change in their medicines.

medicines and diet, it may not be amiss to add the following case of one who has been long subject to severe asthmatic fits.

A gentleman, aged about forty, of a spare make, lively, healthy, and using a great deal of exercise, one day, after too great an exertion of his strength, began to feel a pain in his breast about the *sternum*. Two years after this, he was at times affected with a difficulty of breathing; which continued to increase for several years, and was generally attended with a great flux of humours upon his lungs, and a considerable expectoration of a thick phlegm. In violent fits he found the most immediate relief from bleeding and blistering; and he used, with advantage, vomits of *ipecacuanha*, with the *oxymel scilliticum*, and the *pilule scillitice*, or a solution of gum ammoniac with *acetum scilliticum*. He abstained for several years from wine, malt-liquor, and all flesh-meats, except chicken; and often made his dinner of bread and butter-milk only. He frequently found his breathing made easier, by drinking water with a little vinegar, several times thro' the day.

AFTER he had suffered much by many violent attacks of this disorder, he began to complain of wind in his stomach; and upon vomiting discharged a good deal of tough phlegm. His body became likewise too open, and whatever food or medicine increased this disposition, was hurtful to him. The squill-pills, and the *lac ammoniacum* with the *acetum scilliticum*, did not now agree near so well with him, or do him so much service, as the compound tincture of *castor*, or a solution of *asa fetida* with a little *sal. vol. ammon.* in penny-royal or mint-water: And a bit of broiled meat, with two or three glasses of claret after his meals, agreed better with him than vegetable food, or watery liquors alone; but he found it best to eat little at a time, and often. Now also he found great benefit from the bark, not only in the intervals, but also in the decline of the fits. He took it in decoction, with four ounces of the tincture added to each pound, to the quantity of two table-spoonfuls four times a-day; and, so far from finding it increase his wheezing or difficulty of breathing, he thought it often lessened them, and prevented, or broke the force of smaller paroxysms.

SOON

Soon after he became subject to fits of looseness; he began to spit less than he had done for several years before; and then I observed that blisters, which run longer with him than with most people, did him less service than formerly, when he had a greater expectoration, and no tendency to a looseness. During the violence of the worst fits, he sometimes almost loses his sight, nor is he then able to cough till they begin to abate. At first he brings up a little tough phlegm with great difficulty; but as the constriction in the lungs lessens, he expectorates more freely.

For some years, he had more frequent returns of his disease in summer and autumn than in winter; sudden changes of weather, cold, or fatigue, bring on the *asthma*, which he can sometimes foretell by the paleness of his urine. Flatulent aliments and whatever purges him much, will now, in his best health, occasion a slight fit. Altho' he has been often free from any violent attack for two or three months, yet he seldom breathes in the night so freely as one in perfect health. His pulse is often small, his extremities cold, and face livid, during a severe fit. After bleeding, his pulse becomes fuller and quicker, but does not return to its natural slowness till his breathing is free. The fits are generally over in two or three days; sometimes they last eight or ten; and, after yielding in part, return a second time with more violence. He is commonly worst in the evening, or in the night; and has sometimes exacerbations in the evening and morning. The paroxysms of late are almost always attended with complaints of flatulence in his stomach, and he finds relief as often as he brings up wind. The remedies which in this state have been of most service to his stomach are, the bark, a solution of *asa fetida*, the *emplastrum antihystericum* applied to the epigastric region, and the diet of flesh-meats with claret. A bit of mutton-chop has often given him relief in lesser fits of bad breathing. Observing, that, even in the intervals of the fits, he often breathed with difficulty about three or four in the afternoon, he eat a little mutton-chop, beef-steak, or broiled chicken, between eight and nine in the morning, and dined between one and two on panada with a little claret, or something

equally light. By this means he found the wheezing and difficulty of breathing in the afternoon always much lessened, and sometimes prevented altogether. He often drank near two thirds of a bottle of claret daily, but seldom took above a quarter of a pint of it at once. By this diet, and the constant use of the bark, for above two months, first in tincture and decoction, and afterwards in substance, he not only breathed more easily at all times, but was kept much longer free from the asthmatic fits than usual, not having had an attack of this kind worth mentioning, from the beginning of November till the April or May following; notwithstanding his having been affected several times, during the winter-months, with a cough and a considerable expectoration of thick phlegm.

VIII. A palpitation of the heart.

(1.) WHEN, from a weak or disordered state of the stomach, the heart by sympathy is rendered so irritable, as from very slight causes to be liable to strong palpitations, the most proper remedies are, the tincture of the bark and bitters and moderate exercise. If there be any noxious humours lodging in the stomach, vomits will be proper; and, if the patient be any ways costive, a table-spoonful of the sacred elixir may be given once in two or three nights.

For present relief, spirit of hartshorn, the *tinctura castorei composita*, *spiritus æthereus*, and opiates generally answer best.

(2.) WHEN palpitations proceed from the gouty matter affecting the heart, we ought to trust chiefly to warm stomachic laxatives, to camphire, the volatile salts, the warm *pediluvium*, blisters applied to the legs, or sinapisms to the soles of the feet; and to bleeding, if the patient be of a full habit.

(3.) WHEN palpitations arise from the suppression of some habitual evacuation; if this cannot be restored, the redundant humours are to be carried off by small bleedings, gentle purges, diaphoretics, or issues.

(4.) LASTLY, When palpitations are owing to *polypi* in the heart itself, or in the great blood-vessels opening into it, to accretions of the *pericardium* to the heart, ossified valves, or such like causes; the
disease

disease may be looked upon as incurable ; since, hitherto, we know of no medicines which can remove these causes. However, some relief may be procured by frequent small bleedings, gentle purges, and a cooling, attenuating, light diet ; at the same time, avoiding all viscid, incrassating, and heating aliments, and every kind of exercise that too much quickens the motion of the blood.

IX. AN immoderate discharge of pale urine.

As I have observed above *, that the proximate cause of that discharge of pale water, to which hysteric people are frequently liable, is an increased motion of the secretory vessels of the kidneys ; so there is no medicine that will generally lessen it so soon, or so remarkably as *opium* † ; but as *opium* does not strengthen the kidneys nor remove the several remote causes of this increased secretion, other remedies are required to prevent its frequent returns.

THOSE which have succeeded best with me are, the bark, either in substance or decoction, with some cinnamon added to it ; small doses of the *tinctura rhabarbari amara cum vino*, once in three or four nights ; moderate exercise on horseback or in a chaise, and a diet consisting chiefly of rice, sago, salep, and the lighter flesh-meats roasted, together with a few glasses of claret or red port after meals.

IN cases where the flux of pale urine is attended with hectic heats, I add to the above remedies the *tinctura rosarum*, or elixir of vitriol.

WHEN the increased secretion is, in a great measure, owing to a particular debility of the kidneys, a flannel-shirt will sometimes lessen the quantity of the urine, by increasing the perspiration.

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* See chapter VI. No. 8.

† Altho' in some flatulent or spasmodic cases, *opium* often proves one of the best diuretics, yet it has a contrary effect, when, on account of an uncommon irritation of the nervous system, the urine is secreted in too great a quantity. I know an elderly lady, who is frequently hot and uneasy in the night, and passes a great deal of pale water, whom a dose of *laudanum*, at bed time, always relieves in a great measure of this flux, altho' it seldom procures her good rest.

A tight belt about the loins, or a strengthening plaister applied to them, has been attended with remarkable effects, as will appear by the following cases :

(1.) A gentleman near to forty years of age, troubled with wind in his stomach, and with gouty pains in his feet, in August 1753, was attacked with fits of sickness at his stomach, attended with a quickness of pulse, for which he lay in bed and sweated for several days. After this he began to make great quantities of pale water, infomuch that in the night, he commonly used to pass near an English pint every two hours : After getting up, the quantity began to lessen, and continued to diminish as the day advanced. Notwithstanding the use of the bark, conserve of roses, alum, and several other medicines, for near a fortnight, this flux of urine increased, and, for the two last days, it had been made almost as plentiful in the day as in the night. From a suspicion, that this profuse secretion might arise either from a laxity, or weakness of the renal vessels, or from their being affected with an uncommon alternate motion, about two in the afternoon, a broad posting belt was put about the belly and loins, as tight as the patient could bear it: And altho' all that morning, and the day before, he had made every two hours, at least, three gills of urine, almost as clear as rock-water ; yet, after the belt was on, he voided none for above four hours, and then not quite half a pint. About ten at night, he passed much the same quantity ; but not being able to suffer the belt in bed, it was removed, and the flux of urine returned in the night-time, tho' not to that degree it had done for several nights before. From this time, by keeping to the belt, riding out in a chaise, and returning to the use of light flesh-meats, which, on account of a quickness in his pulse and great thirst, he had almost wholly abstained from, the flux of urine daily lessened, and the patient recovered.

(2.) A gentleman, upwards of thirty years of age, after having been in a flow fever, attended with rheumatic pains, for ten or twelve days, began (November 20. 1745) to make a great deal of pale water, and chiefly in the night-time. This disorder, notwithstanding

standing the use of the bark, *tinctura rosarum*, and other remedies, continued without any sensible abatement till the 24th of December, when a large plaister of the *emplastrum defensivum* being applied to the *os sacrum* and loins, had so good an effect that, on the night following, he made no water till after he had been three hours in bed; tho', for some time before, he had seldom lain an hour and a half without a call. The second night he was above four hours in bed before he was disturbed, and the quantity of urine thro' the whole night did not exceed five gills; whereas, for several weeks before, it had commonly amounted to three English pints a-night, and sometimes to much more. In less than a week, from the time he had applied the plaister, the urine returned to its natural colour and quantity, and the patient soon recovered his strength.

THE same gentleman, in November 1750, after a fever and cough, was again attacked with his old distemper, upon which he had immediately recourse to the *emplastrum defensivum*, which, tho' it seemed somewhat to restrain the flux for the first night, yet had no effect afterwards. But it is to be observed, that, on this relapse, neither the bark nor the other strengtheners had been used before the plaister.

(3.) MR J. P. aged above 50, after a tedious fever in July 1758, began to make in the night great quantities of pale water, which much retarded his recovery. After using the bark, claret, and other remedies, I advised him to apply to his loins the same strengthening plaister which I had found so serviceable to the last mentioned patient; by means of which, the flux was in a few days sensibly diminished, altho' not quite stopped for several weeks.

X. PERIODICAL headachs.

(1) WHEN these arise from a disordered state of the stomach, the best medicines are vomits, stomachic laxatives, and bitters. If there is an acid in the stomach, the testaceous powders, *magnesia alba*, or lime-water will be of great use.

(2) WHEN periodic headachs proceed from a rheumatic or gouty humour affecting the small vessels or nerves of the *pericranium*, or other

other parts of the head; the properest remedies are blisters applied to the head or legs; issues in the head or neck; the warm *pediluvium*, with dry friction of the legs and feet, and frequent doses of sacred tincture *.

IN a violent pain of the head from a rheumatic humour, after several other remedies had failed, I have seen good effects from fifteen or twenty grains of *gum. guaiac.* with ten grains of *sal. vol. ammon.* given in a bolus at bed-time, and repeated for several nights.

THE *pulvis fol. asari*, used as a sternutatory, has sometimes cured obstinate headaches, by making a large evacuation from the vessels of the nose.

(3.) WHEN periodic or frequently returning pains of the head are owing to a peculiar weakness or delicacy of the nerves of that part, rendering them liable to be affected by slight causes, we must attempt to relieve the patients by the bark, chalybeates, moderate exercise, and daily washing of the head with cold water. Here also, some of those medicines, commonly called nervous, may be of use, such as camphire, musk, and valerian. The last of these has been commended as a kind of specific in obstinate hemicranias †; and I have found it serviceable in removing a confusion of the head, with which an epileptic patient, of very delicate nerves, was almost constantly affected; as well as in lessening or protracting the returns of the convulsive fits. The valerian in this case was given in the form of an electuary, and to the quantity of three drachms daily.

(4.) WHEN headaches are regular as to their periods, vomits should be given an hour and an half, or two hours, before the returns of the fits, and the bark between them.

I have known a violent *hemicrania*, which returned regularly at a certain time of the day, in a good measure prevented by taking the

* The following effect of an extraordinary dose of sacred tincture was communicated to me by a physician of character. A lady, afflicted with a rheumatic pain in her head, by mistake drank over night, at once, near an English pint of sacred tincture. Next day, she purged seven times, and, for three days after, salivated, as if she had taken mercury; but was entirely cured of the pain of her head.

† See Fordyce de Hemicrania.

the following draught an hour before the coming on of the pain.

R. Laud. liquid. gutt. xl.
Tinct. ipecacuan. gutt. xlv.
Sp. Minderer. unc. fs.
Aqu. rosar. unc. i.
Sacch. alb. drach. ii. Misce.

THIS medicine, while it lessens the pain, seldom fails to raise a plentiful sweat.

(5.) WHEN headaches, whether regularly preriodic or not, arise from a suppression of the *menfes*, we must endeavour to restore this evacuation; but if that cannot be done, bleeding, especially at the ancles, perpetual blisters or issues in the head or neck, and laxatives, are the best remedies.

(6.) WHEN the returning pain has been attended with a swelling of the part, after other medicines had failed, mercurial laxative pills have succeeded *.

(7.) To relieve present pain in violent periodic headaches, the best remedies are, the warm *pediluvium*; flannel cloths, wrung out of hot water, or a hot decoction of rosemary, and applied to the shaven head; the *spiritus æthereus* applied in the hollow of one's hand to the pained part, and kept there for some minutes; large doses of *laudanum*, and, in some cases, leeches put to the temples.

COLD water will give ease in some headaches, while hot applications do most service in others. In like manner, shaving the head relieves some patients, but is hurtful to others.

IT

* A lady, between 25 and 30 years of age, of a thin habit, and delicate constitution, was several years since attacked with a *hemicrania*, which returned commonly every afternoon; at which time, that part of her forehead which was affected was often sensibly swelled. She kept a perpetual blister on her head for many months, used sacred elixir and tincture, *pulvis asari* as a sternutatory, and other remedies, without any benefit; insomuch, that after a twelvemonth, her complaint was rather worse than ever. She was then advised to take from twelve to sixteen grains of the *pilule mercuriales laxantes* every other night. By the use of these pills, a gentle salivation was raised, and kept up for about twelve days, which entirely removed the headach; nor has she had any return of it since.

Another person, between 40 and 50 years of age, having a constant headach with a swelling about the *occiput*, was cured by the same remedy, but without raising any salivation.

It may be proper to observe, that in all violent headaches, we ought to begin the cure with bleeding, either by applying leeches to the temples, or opening the artery there. If the patient be plethoric, a larger quantity of blood may be taken from the jugular vein.

MODERATE exercise is generally useful in periodic headaches, from whichever of the foregoing causes they may arise; but the proper time for it is in the intervals of the fits.

XI. Low spirits.

HYPPOCHONDRIAC and hysteric patients are commonly affected with this complaint, in a greater or less degree. In general, exercise and the cold bath are among the best remedies. But to be more particular:

(1.) WHEN low spirits are owing to a weak state of the nerves of the stomach and bowels, the tincture of the bark and bitters, chalybeates, aromatics, a proper diet * and riding, will do most service.

(2.) WHEN they arise from obstructions in the hypochondriac viscera, or a foulness of the stomach and intestines, the most proper medicines are, aloetic purges, Harrowgate waters, and soluble tartar. I commonly prescribe the soluble tartar in the following manner:

R. Tart. solub. drach. ii. ad unc. fs.

Solve in aqu. fontan. unc. viii.

Cui adde aqu. cinnamon. f. v.

Syr. violar. ana unc. i. Misce.

THIS solution is to be taken at two or three draughts, either every morning, or only once in two days, and to be continued for several weeks.

DR MUZZEL has published, some years since, several instances of the success of the soluble tartar in madness and melancholy. In cases of low spirits, I have found it cool the patients, dispose them to sleep, and quiet the hurry of their spirits; but it sometimes be-

comes

* See above, p. 633. &c.

comes hurtful, by increasing flatulence, and occasioning a faintness: and, as far as I have observed, the soluble tartar is more useful in maniac or melancholic disorders, proceeding from noxious humours in the *primæ viæ*, than in those which are owing to a fault in the brain.

(3.) WHEN low spirits proceed from a suppression of the *menfes* or hæmorrhoids; if these evacuations cannot be restored, some others must be substituted in their place: But nothing has such sudden good effects as bleeding *.

(4.) LASTLY, When low spirits or melancholy have been owing to long-continued grief, anxious thoughts or other distress of mind, nothing has done more service than agreeable company, daily exercise, especially travelling, and a variety of amusements.

* A gentlewoman, aged fifty, soon after the *menfes* had left her, was seized with a cough, and sometimes with a slight hæmoptoe. This last symptom went off in a few months, but the cough lasted above three years; and upon its ceasing, she began to be much troubled with wind in her stomach, low spirits, a confusion in her head, and a want of sleep. In this condition she continued for several months; during which, these complaints increased, notwithstanding the free use of warm, carminative, aromatic, chalybeate, and antihysterical medicines. A blister applied to her head lessened the confusion in it, and procured her better rest for a few nights. Believing that as the cough was, in some measure, a consequence of the suppression of the *menfes*, so the wind in the stomach and low spirits were owing to the nerves of this organ being disordered by that matter which used to be thrown off by the lungs; altho' her pulse was neither full nor quick, I ordered ten ounces of blood to be taken from the arm; immediately after which, her spirits were relieved, the confusion in her head and watching were removed, and the flatulent symptoms were much lessened.

This person afterwards, upon the return of the same symptoms, has found bleeding do her more service than any other remedy.

CASES of the Remarkable Effects of BLISTERS in lessening the quickness of the pulse in coughs, attended with infarction of the lungs and fever.

• ONE of the most natural effects of blistering plaisters, when applied to the human body, is to quicken the pulse, and increase the force of the circulation. This effect they produce, not only by means of the pain and inflammation they raise in the parts to which they are applied, but also because the finer particles of the *cantharides*, which enter the blood, render it more apt to stimulate the heart and vascular system.

THE apprehension that blisters must in every case accelerate the motion of the blood, seems to have been the reason why some eminent physicians have been unwilling to use them in feverish and inflammatory disorders, till after the force of the disease was a good deal abated, and the pulse beginning to sink. However, an attentive observation of the effects which follow the application of blisters in those diseases, will shew, that instead of increasing, they often remarkably lessen the frequency of the pulse. This I had occasion formerly to take notice of †, and shall now evince more fully by the following cases.

I. A widow lady, aged about fifty, was seized (December 1755) with a bad cough, oppression about her stomach and breast, and a pain in her right side, tho' not very acute. Her pulse being quick, and skin hot, some blood was taken away, which was a good deal fizy: Attenuating and expectorating medicines were also prescribed. But as her complaints did not yield to these remedies, I was called on December 26th, after she had been ill about ten days; at which

4 X 2

time

* Read Feb. 16. 1758.—Vide Phil. Tran. vol. 50. part 2. p. 569.

† See above, p. 241.

time her pulse beat from 96 to 100 times in a minute, but was not fuller than natural. I ordered her to lose seven or eight ounces more of blood, which, like the former, was fizy; and next day, finding no abatement of her complaints, I advised a blister to be applied, in the evening, to that part of her right side which was pained. Next morning, when the blister was removed, the pain of her side was gone, and her pulse beat only 88 times in a minute, and in two days more it came down to 78. However, after the blistered part became dry, the pulse rose in one day's time to 96, and continued between that number and 90 for four days; after which I ordered a large blister to be put between her shoulders. When this plaster was taken off, her pulse beat under 90 times in a minute; and next day it fell to 76, and the day after to 72. The cough and other symptoms, which were relieved by the first blister, were quite cured by the second.

II. JOHN GRAHAM bookbinder in Edinburgh, aged thirty-seven, of a thin habit of body, formerly subject to coughs, and thought to be in danger of a *phthisis pulmonalis*, having exposed himself unwarily to cold in the night-time, was, about the end of January 1756, seized with a bad cough and feverishness; for which he was blooded, and had a diaphoretic julep, a pectoral decoction, and a mixture with *gum. ammoniacum* and *acetum scilliticum*, given him by Mr James Ruffel, surgeon-apothecary in this place. On the 12th of February, after he had been ill above a fortnight, I was desired to visit him. He seemed to be a good deal emaciated; his eyes were hollow, and his cheeks fallen in: He was almost constantly in a sweat; coughed frequently, and spit up a great quantity of tough phlegm, somewhat resembling *pus*: His pulse beat from 112 to 116 times in a minute. In this condition I ordered immediately a blister to be applied between his shoulders, which lessened in some degree his cough and spitting, as well as the frequency of his pulse; but the blistered part no sooner began to heal, than he became as ill as before, and continued in this bad way nine or ten days, gradually wasting with continued sweats, and a great

great spitting of a thick *mucus*. During this time he used *tinctura rosarum*, and the mixture with *gum. ammon.* and *acet. scillit.* without any sensible benefit, and had six ounces of blood taken away, which was very watery, and the *crassamentum* was of a lax texture. In this almost desperate condition, another blister, larger than the former, was put between his shoulders, which remarkably lessened his cough and spitting, and in two or three days reduced his pulse to 96 strokes in a minute. After this he continued to recover slowly, without the assistance of any other medicine, except the *tinctura rosarum*, and the mixture with *gum. ammon.* and *acet. scillit.* and at present he enjoys good health.

III. MRS —, aged upwards of forty, who had for several years been subject to a cough and spitting in the winter-months, was, in October 1756, seized with those complaints in a much greater degree than usual; to remove which she was bled, and got some attenuating and pectoral medicines from Mr John Balfour, surgeon-apothecary in Leith. I was called on November 11th, after she had been ill several weeks, and found her in a very unpromising condition. She had a frequent and severe cough, with great shortness of breath and a wheezing; her lungs seemed to be quite stuffed with phlegm, of which she spit a vast quantity every day, and of such an appearance, that I was apprehensive it was, in part at least, truly purulent. When she sat up in a chair, her pulse beat above 130 times in a minute. She had a considerable thirst, and her tongue was of a deep red colour, with a beginning aphthous crust on some parts of it. She was so weak, and her pulse so feeble, that there was no place for further bleeding: A blister was therefore applied to her back, November 11th, which somewhat lowered her pulse, and lessened the shortness of breathing and quantity of phlegm in her lungs. November 16th, a second blister was laid to her side, which gave her still more sensible relief than the former, and reduced her pulse to 114 strokes in a minute. November 25th, a third blister was applied to her back, by which her cough and wheezing were rendered considerably easier, and the phlegm which she

she spit up, lost its purulent appearance, became thinner, more frothy, and was much less in quantity. Her pulse beat now only 104 times in a minute. After this her cough and spitting increasing again, she had, on the 20th of December, a fourth blister applied to her back, which, like the former, did her great service. Her stomach being extremely delicate, I scarce ordered any medicines for her all this time, except a cordial julep, with *spir. volat. oleos. tincture of rhubarb* as a laxative, and a julep of *aqu. rosar. acet. vin. alb.* and *syr. balsam.* of which last she took two table-spoonfuls twice or thrice a-day in a quarter of a pint of lint-seed tea. After the fourth blister, she drank for some time a cup-full of *infusum amarum* twice a-day, and continued to recover slowly: And tho' during the remaining part of the winter she was, as usually, a good deal troubled with a cough, yet in the spring she got free from it, and is now in her ordinary health.

IV. CHRISTIAN M'EWEN, aged twenty-one, had laboured under a cough, thick spitting, pain of her breast, and pains in her sides affecting her breathing, for about a twelvemonth: And after getting, by proper remedies, in a good measure free from those complaints, her cough, from catching a fresh cold, increased to a greater degree than ever, became hard and dry, and was attended with a constant difficulty of breathing, pain in her left side, and headach. After having been seven or eight days in this condition, she was admitted into the Royal Infirmary, January 9th, 1757. As her pulse was small, tho' very quick, *viz.* beating 130 in a minute, I thought it unnecessary to bleed her, as from former experience I did not doubt but that blistering alone would relieve her: I ordered, therefore, a large blister to be applied to her left side, where she complained of pain, and prescribed for her the following julep:

R. *Aqu. menth simp. spirit. Minderer. ana unc. iij. acet. scillit. unc. i. sacchar. alb. unc. ij. Misce; cap. coch. ij. ter in die.*

She was also desired to breathe frequently over the steam of hot water, and to drink lintseed-tea.

JANUARY 10th. Her pulse beat only 112 times in a minute, and
was

was somewhat fuller than on the 9th. The blister was not removed till late in the evening, and made a plentiful discharge. The cough having been so severe last night as to keep her from sleep, I ordered her the following anodyne draught:

R. Spirit. Minderer. unc. ss. acet. scillit. drach. i. fyr. papav. alb. drach. vi. Misce;
cap. hor. somni.

JAN. 11th. The cough easier last night; difficulty of breathing less; pulse 108 in a minute. Ordered the anodyne draught to be repeated, and the use of the julep, with *acet. scillit.* to be continued.

JAN. 12th. Pulse slower; cough and pain of the side easier; but still complains of a headach.

JAN. 13th. Pulse 94 in a minute; cough continues easier in the night, but is troublesome in the day-time.

JAN. 14. Every way better; pulse only 80 in a minute. As her cough is still bound, ordered her, besides the medicines above mentioned, a pectoral decoction of *rad. alth. &c.*

JAN. 15th. Cough and other complaints in a great measure removed: Pulse 65 in a minute.

FROM this time her cough gave her little trouble; but on the 18th she complained of a pain in the *epigastrium*, with sickness at stomach, want of appetite, and a giddiness in her head, which were considerably relieved by a vomit, *infusum amarum*, and stomachic purges; and were almost wholly cured by the return of her *menfes* on the 5th of February, after an interval of eight weeks.

V. A girl twenty-one months old, who had (December 1756) a great load of the small-pox, and not of a good kind, with a cough and obstructed breathing, was, on the seventh day from the eruption, blistered on the back; by which the pulse was lessened from 200 to 156 strokes in a minute. Next day her legs were also blistered, and the pulse thereby fell to 136. But the child's lungs being much oppressed, and her throat being so full of pustules that she could scarce swallow any thing, she died towards the end of the ninth day.

I could add several other cases of the remarkable effects of blisters, in lessening the quickness of the pulse in coughs attended with fever, pain in the side, and pituitous infarction of the lungs: but those above may be sufficient to put this matter out of doubt, as well as to remove any prejudice that may still remain against the free use of so efficacious a remedy.

IN a true peripneumony, especially where the inflammation is great, repeated bleeding is the principal remedy, and blisters early applied are not so proper. But when the peripneumony is of a mixed kind; when the lungs are not so much inflamed as loaded with a pituitous matter; when bleeding gives but little relief; when the pulse, tho' quick, is small; when the patient is little able to bear evacuations, and the disease has continued for a considerable time: In all these cases blistering will produce remarkable good effects, and, far from increasing, will generally lessen the frequency of the pulse and fever more speedily than any other remedy.

ON the other hand, when the fever and frequency of the pulse proceed from a true inflammation of the lungs, from large obstructions tending to suppuration, or from an open ulcer in them, blisters will be of less use, nay, sometimes will do harm, except in the last case, when they, as well as issues and setons, are often beneficial, tho' seldom able to compleat a cure. But as in pituitous infarctions of the lungs, with cough and fever, repeated blisters applied to the back and sides are far preferable to issues or setons, so these last seem most proper in an open ulcer of the lungs. The former make a greater and more sudden derivation, and are therefore adapted to acute cases; the latter act more slowly, but for a much longer time, and are therefore best suited to chronic diseases. Further, while blisters evacuate chiefly the serous humours, issues and setons generally discharge true purulent matter, and on this account may be of greatest service in internal ulcers.

IN what manner blisters may lessen the fever and frequency of the pulse, attending internal inflammations, I have elsewhere endeavoured to explain *; and shall only add here, that in the cases
above

* See above, p. 241.

above recited, where the quick pulse and feverishness proceeded more from a pituitous infarction than a true inflammation of the lungs, blisters, by relieving this organ, in some measure, of the load of humours oppressing it, would render the circulation thro' its vessels freer, and consequently lessen the quickness of the pulse, and other feverish symptoms.

It may not, however, be improper briefly to point out the reason why blisters, which have been observed to be remarkably efficacious, even when early applied, in pleurifies *, are less so in true peripneumonies. This difference, I imagine, may be accounted for from there being no immediate communication between the pulmonary vessels and those of the sides and back, to which the blisters are applied; whereas the *pleura* and intercostal muscles are furnished with blood-vessels from the intercostal arteries, which also supply the teguments of the *thorax*: so that while a greater flow of ferous humours, and also indeed of red blood, is derived into the vessels of the external parts, to which the vesicatories are applied, the force of the fluids in the vessels of the inflamed *pleura*, or intercostal muscles, must be considerably lessened. Further, as the intercostal muscles and *pleura* are, as well as the teguments of the *thorax*, supplied with nerves from the true intercostals, blisters applied to the back and sides may perhaps, on this account, also have a greater effect in relieving inflammations there than in the lungs, which have nerves from the eighth pair, and from the intercostals improperly so called.

Edinburgh, May 23d,

1757.

* Dr Pringle's Observations on the diseases of the army, part 3. chap. 2.

Extract of a Letter from Dr WHYTT, Professor of Medicine in the University of Edinburgh, and F. R. S. to Dr PRINGLE, F. R. S.

Edinburgh, Nov. 10th, 1757.

WHAT you remark with regard to blisters being freely used by the physicians at London, in the cases mentioned in the paper I last sent you, is very just, and indeed what I knew. But altho' their efficacy in such circumstances is now generally acknowledged both in England and Scotland, yet I do not remember that their remarkable quality in lessening the quickness of the pulse has been particularly attended to. This, therefore, I thought it might not be amiss to ascertain by a few careful observations.

I agree intirely with you, as to the use of blisters in fevers; being of opinion, that when there is no particular part obstructed or inflamed, they are of little service, and sometimes hurtful, unless perhaps towards the end, when the pulse begins to sink. Nay, in fevers, where the substance of the brain is affected, and not its membranes, I have never found any sensible benefit from blisters: And I always suspect the brain itself affected, when a fever and *delirium* come on without any preceding head-ach, or redness in the *tunica albuginea* of the eyes. This kind of fever I have met with several times, and have observed it to be generally fatal.

OBSER-

O B S E R V A T I O N S

ON THE

D R O P S Y

In the B R A I N.

Never before published.

OF SEVERAL

ON THE

OF R O P S Y

IN THE R A I N

OBSERVATIONS

ON THE

Most frequent Species of the HYDROCEPHALUS INTERNUS,

VIZ.

The DROPSY of the VENTRICLES of the BRAIN.

THE *hydrocephalus*, or dropfy of the head, is either external or internal. The former has its feat in the cellular substance, between the skin and the *pericranium*, or between this membrane and the skull. In the internal *hydrocephalus*, the water is sometimes collected between the *cranium* and *dura mater*, or between this last and the *pia mater*; but most commonly it is found in the ventricles of the brain, immediately below the *corpus callosum*: And this is not only the most frequent and fatal species of the *hydrocephalus*, but also that with which medical writers seem to have been least acquainted.

HIPPOCRATES, in his second book *de morbis*, has enumerated the signs of water in the brain, as his words have been rendered by all the translators. But *ἐπὶ τῷ ἐγκεφαλῷ* more properly signifies upon than in or within the brain; and that Hippocrates only speaks here of water lodged between the *dura mater* and brain, can scarcely

scarcely be doubted, since he proposes to evacuate it, by making a perforation in the upper part of the *cranium*, *προς τον εγκεφαλον*; which operation could have been of no use, had the water been contained within the brain itself.

CELSUS has only mentioned briefly the *hydrocephalus externus*, or dropy of the teguments of the head *. Aetius and Paulus Aegineta go a little farther; for when they treat of this disease, they observe that water is sometimes found between the skull and the membranes of the brain.

HIERONYMUS MERCURIALIS, who flourished in the beginning of the sixteenth century, mentions the collection of water in the ventricles of the brain as a thing that may possibly happen; but adds, that in such a case an apoplexy must be the consequence †.

WEPFER has collected several cases from different authors, in which water was found in the cavities of the brain ‡; and the celebrated Boerhaave mentions such a disorder as one species of the *hydrocephalus* ||. But none of these authors, nor indeed any other that I have met with, who wrote before them, have favoured us with the signs by which we may distinguish a dropy of the ventricles of the brain from other diseases affecting that organ.

M. PETIT, in a short paper on the *hydrocephalus*, published in the Memoirs of the academy of sciences for the year 1718, observes, that in all the bodies which he had opened, he never found water any where within the *cranium*, but in the ventricles of the brain; and therefore supposes the other species of internal *hydrocephali* to be very rare.

THE symptoms of a dropy in the cavities of the brain, according to that justly esteemed author, are, in the beginning, slight convulsions of the mouth and eye-lids, biting of the lips, grinding of the teeth, and picking of the nose, as in the case of worms. The patients are either costive or have a purging, and sometimes a vomiting.

* De medicina, lib. 4. cap. 2.

† Opuscula aurea, lib. de morb. puerorum.

‡ H. apoplecticorum.

|| Boerhaave Aphorism. § 1218.

miting. They are more or less drowsy, according to the quantity of water within the brain. They grow languid, feeble, sad, and pale; the eyes look dull, the pupil dilates, the futures of the skull open, and its bones become soft. The forehead rises, the eyes seem to be protruded out of their orbits, the head swells so as sometimes to burst, and the patient dies soon after.

ALTHO' this account of the symptoms of the *hydrocephalus internus* be much more just than what is to be met with in any author before M. Petit; yet still it is so far incompleat, that I may venture to say, that it will not be found sufficient to distinguish a dropsy within the brain, unless when it is attended with a swelling in the head.

M. PETIT mentions slight convulsions of the mouth and eyelids in the beginning; whereas I have never seen any convulsions till towards the end. He says, the patients are always more or less drowsy; but I, on the contrary, have often observed them more watchful at first, altho' in the advanced state they not only become drowsy but comatose. He informs us, that he never saw the water collected any where, but in the ventricles of the brain. Now, were this the case, it is certain that the opening of the futures and swelling of the head could not happen but to the youngest infants, who, by the bye, are not so subject to this kind of *hydrocephalus* as children of two years old and upwards; for, of about twenty patients whom I have seen die of this distemper, one only was under half a year old, the rest between two and sixteen; who all went off without any swelling of the head, opening of the futures, or protrusion of the eyes.

LASTLY, M. Petit has taken no notice of the aversion to light, squinting, the variations of the pulse, and the degree of feverish heat, which, as we shall afterwards see, are the surest diagnostics of the disease.

M. LE DRAN, who wrote after M. Petit, has described the *hydrocephalus internus* in such a manner as would make one believe he had never seen the distemper, except when it happened
to

to be joined to a collection of water between the *cranium* and brain *.

DR DONALD MONRO, in his treatise of the dropfy, has well enumerated the several kinds of the *hydrocephalus*: But by the symptoms he mentions, of the internal kind, we shall be hardly able to distinguish it from several other disorders of the brain, as he himself has very justly remarked.

It may seem strange, that a dropfy of the ventricles of the brain, which in our days so frequently occurs, should have been altogether unknown to the ancients, and so little attended to by most of the moderns. The reason may be, that those patients who were carried off by this disease have been generally supposed to die of a fever ending in a *coma*; and in such cases the head is seldom opened.

ALTHO' a dropfy of the ventricles of the brain does very rarely occasion any opening of the futures, or swelling of the head †; yet in most cases it may be easily distinguished from every other disorder, by the following symptoms, which with the greatest care I have collected, in attending about twenty patients in this disease.

An

* See his Operations in surgery, article of the Dropfy.

† VESALIUS gives an account of a child of two years old, whose head was greatly enlarged, and in the ventricles of whose brain he found nine pounds of water: But this is an extraordinary case; and it is probable the water began to be collected soon after the child's birth, and before the futures of the skull could offer any considerable resistance to its pressure. I shall only add here, that I have not only never observed any increase of the size of the head in the species of *hydrocephalus* of which I now treat, but that it is an error, though a common one, to imagine, that those children who have big heads are most liable to this disease; for of all those whom I have attended, few or none were remarkable for the largeness of their head, but several had been very sprightly, and of a delicate make.

An Account of the SYMPTOMS in the DROPSY of the Ventricles of the BRAIN.

FIRST STAGE.

CHILDREN who have water in the ventricles of the brain begin to have many of the following symptoms, four, five, or six weeks, and in some cases much longer, before their death.

AT first they lose their appetite and spirits; they look pale, and fall away in flesh; they have always a quick pulse, and some degree of fever. In some cases I have seen a *hydrocephalus* attended with a considerable degree of fever, which had frequent remissions, but without any order or regularity: In other cases the paroxysms came on pretty regularly in the evening, and then the disease was taken for a slow irregular nervous fever, or for one occasioned by worms. At this time, in children of five years and upwards, I have found the pulse at a hundred and ten, in others at a hundred and twenty, and in a few cases at a hundred and thirty, or even at a hundred and forty strokes in a minute; but rarely ever so full as to indicate bleeding.

IN others the quickness of the pulse and heat of the skin were not so considerable; but I do not remember to have seen any patient who had not some degree of fever in this, which I call the first stage of the disease.

WHILE the feverishness continues or increases, they lose their appetite more and more; their tongue is often white, sometimes it is remarkably clean, and towards the end of the disease acquires an aphthous redness. They are thirsty, and frequently vomit once or twice in a day, or once in two days. They complain of a pain in the crown of their head, or in the forehead above their eyes. They are commonly constipated, tho' sometimes they have returns of a looseness. When bound, they are not easily moved by a purge; sometimes they are troubled with gripes. Their spirits being low, they in-

cline mostly to lie in bed, altho' they are often more disposed to watching than to sleep. They cannot easily bear the light, and complain when a candle is brought before their eyes. They are observed to pick their nose, and in their sleep to grind with their teeth, as in the case of worms.

THESE are the symptoms of the first stage, during which it is very hard to distinguish this dropfy of the brain from a flow irregular fever occasioned by worms, by some other disorder in the bowels, or by some other cause. In the second stage, the symptoms enable us, with some certainty, to discover the nature of the ailment. But before I proceed to enumerate them, I shall just observe, that I never had but two patients who had not the vomiting during either the first or second stage. One of these was a girl of eight years of age, who, tho' she had an aversion to food, yet never threw it up but once, and that was on the third day before her death; nor did she ever complain of a headach till twelve or fourteen days before she died; whereas this last symptom, for the most part, begins three or four weeks, and in some cases several months, before the end of the disease: She also could bear the light better than any I have seen. The other, who had no vomiting, was a boy of eleven years; he had little headach, altho' he lay much in bed, and did not like to be moved. But in general, the vomiting once or twice a-day, or once in two or three days, the headach *, and the aversion to light, are the symptoms which in the first stage of this kind of *hydrocephalus* characterize it most.

The Symptoms of the SECOND STAGE.

I date the beginning of the second stage from the time the pulse, from being quick but regular, becomes flow and irregular. This
some-

* The headach not only in this, but the succeeding stages, is in some moderate, in others severe; in which last case, it is always easiest in the morning and worst at night; and these patients have commonly a great aversion to food.

sometimes happens about three weeks, often a fortnight or less, before the death of the patient.

IN this stage the pulse is commonly not only much slower than it was before, but often more so than in health. In a girl of thirteen, the pulse, which for a fortnight beat above a hundred times in a minute, about nine days before she died, fell to eighty-four, next day to seventy, and the day after to sixty, becoming always the more irregular the slower it was. In a youth of sixteen the pulse, which for several weeks had been feverish, on the fifteenth day before his death, beat only sixty-eight in a minute; two days after, it fell under sixty, and once to fifty.

A boy of nine years of age, fifteen days before he died, had a pulse from seventy to seventy-five in a minute, and irregular. In another of four years, the pulse fell to eighty-eight on the ninth day before his end. In a girl of seven years old, on the fifteenth or sixteenth day before her death, the pulse beat a hundred and fifty times in a minute; next day, it became slower than natural and irregular; for five or six days after this, it was from eighty to eighty-six in a minute.

IN two other children, who were less feverish in this stage, the pulse from a hundred fell below eighty. I have never seen a patient with water in the ventricles of the brain, whose pulse did not come down to its natural state, or very near it, except one. This was a girl of about seven, whose pulse, after being for several weeks about a hundred and thirty in the forenoon, and a hundred and forty in the evening, a fortnight before her death, fell two or three strokes under a hundred; yet neither her heat nor thirst, nor other complaints abated, altho' her pulse had fallen above thirty in a minute.

IN this distemper it is observable, that when the pulse is nearly as slow, or slower than natural, it is always irregular or unequal, both as to the strength and the interval of the strokes. When it grows quicker, the irregularity lessens; and when it becomes very quick, it is then most equal and regular. Farther, it deserves notice, that, altho' in the second stage the pulse becomes much slower than

it was before, the heat of the skin continues much the same, and sometimes seems rather to increase.

I have insisted the longer on the state of the pulse in this period, as from thence we can learn the surest *diagnostic*.

DURING the second stage, most of the symptoms mentioned in the first continue. The sick are then unable to sit up, tho' generally they sleep little, till towards the end of this period, when they begin to grow drowsy. They moan heavily, yet cannot tell what ails them. Their eyes are often turned towards their nose, or they squint outwards, and sometimes they complain of seeing objects double. Some, towards the end of this stage, grow delirious, and cry out in a wild manner, as if they were much frightened: About this time also, or later, they frequently void either real worms, or some substance like worms in a dissolved state; yet this discharge gives no relief to the patient, and only helps to deceive the less experienced practitioner with regard to the nature of the disease.

THE urine in this, as well as in the other stages, varies; it has often a large sediment, sometimes none at all; but most commonly it deposits one of a light consistence and a white colour. In several I have observed the urine have a large furfuraceous sediment, till within a few days of their death, when it had no separation.

THE breath has now, but especially in the last stage, such a sickish and offensive smell, as I do not remember to have observed in any other distemper. During the second as well as the first stage, the patients are often, for some days, or parts of days, much easier than at other times.

The Symptoms of the THIRD STAGE.

WHEN the pulse (which for some time was nearly as slow or slower than in a healthful state) rises again to a feverish quickness, and becomes regular, the third and last stage may be said to begin.

THIS change in the pulse is observed five, six, or seven days before death. In two patients only the pulse did not become more frequent

frequent till two days before they died ; and in two others it began to grow quicker nine or ten days before that event.

As the time of this change in the pulse is different in different patients, so is the degree of its quickness. In some it rises gradually from below seventy, eighty, or ninety in a minute, to a hundred and twenty, a hundred and forty, a hundred and seventy, and sometimes above two hundred, before they expire. In others the pulse gets up more suddenly, in one day perhaps from a hundred to a hundred and fifty. In the last stage, after the pulse grows quicker, it does not keep constantly to the same measure, but will be often a good deal slower for part of a day, and quicker all the rest. The pulse beats generally faster on the day they die than at any time before. In one of those whom I attended, it beat above two hundred and ten times in a minute. I never knew any go off in this disease whose pulse did not rise to near a hundred and thirty strokes in that time.

IN the third stage, the patient, who before was little disposed to sleep, becomes then drowsy and comatose. When roused, he utters only a few incoherent words, and appears to be insensible. The beginning of the *coma* is uncertain ; it is often about the end of the second stage before the pulse grows quicker for the second time ; but in a few cases I have known this quickness of the pulse come on before the patients become comatose.

FREQUENTLY one eye-lid loses its motion, and afterwards the other becomes also paralytic. About this time, or rather sooner, the pupil of one or both eyes ceases to contract, and remains dilated in the greatest light. But the time of this symptom varies much : In some it happens five, six, or seven days, in others only two or three days, before they die. Three or four days before the death of a boy of five years old, I was surprised to find the pupils, which had been much dilated before, no larger than natural. At first I flattered myself, that the distemper had taken some favourable turn ; but was soon undeceived ; for, upon giving the child a spoonful of weak cinnamon water, with some drops of *spiritus volatilis oleosus*, the pupils became as wide as they had been the day before. In less than half an hour after, they contracted again ; but immediately dilated upon holding some spirit of *sal. ammoniacus* to his nose. I have

have since observed the same interchanges in the pupils of a boy of four years old, on the third day before he died. In this case the pupils not only were enlarged, by giving him a spoonful of wine, or holding volatile spirits to his nose, but also by so small a *stimulus* as my lifting up his eye-lids, which had lost all their motion, and had fallen so far down as to cover near the half of the eye. Before they are seized with the *coma*, they sometimes complain of seeing strange and frightful objects. A day or two before death, the *tunica conjunctiva* of one or both eyes frequently becomes inflamed; but they generally continue to hear for some days after they are blind.

IN this stage, the patients are sometimes observed to be constantly raising one of their hands to their head; and are generally troubled with convulsions of the muscles of the arms, legs, or face, as well as with a *subfultus tendinum*. In a girl of thirteen, the day before she died, the hands were strongly bent inwards by a fixed spasm of their muscles. A youth of sixteen, who when in health had been liable to spasms, about the end of the second stage began to be affected once or twice a-day with a cramp in one of his arms, which ascended to his throat, and often prevented his speaking for some minutes. One of the cheeks will twice or thrice in a day grow hot and red, while the other, with the lips, remains pale and cold. These flushings generally appear two, three, or four days before death. In a boy of five years old, one side of both his arms became frequently red, while the other side never changed its colour. After death, the arms and breasts have been seen of a deep purple colour.

I had one patient who, four days before he died, bled once and again at the nose.

THOSE who have been costive before, often become loose in the third stage, and complain of gripes. A day or two before death, the patient either swallows with difficulty, or not at all. Lastly, the respiration grows more frequent and laborious; and in some there is a considerable pause after every expiration. This kind of breathing

breathing I have also observed in those who have died of an apoplexy, arising from a suppression of urine.

UPON opening the heads of ten of those patients from whom I have collected the symptoms above mentioned, I found in all of them a clear thin fluid in the anterior ventricles of the brain, immediately below the *corpus callosum*. There was frequently the same kind of liquor in the third and fourth ventricles; but whether this is always the case, I cannot say, as I had not attended sufficiently to this circumstance. I never met with water between the *dura mater* and the brain, between the hemispheres of the brain, or immediately above the *corpus callosum*. Altho' there seems to be a communication between the two anterior ventricles; yet, in two cases, I found one of them much distended, while the other contained but little water.

THE quantity of water contained in the ventricles of the brain was generally from two ounces to five; but I have been told of one case in which it amounted to near eight ounces. This fluid does not coagulate with heat, like the *serum* of the blood, or the lymph that is found in the *pericardium*, or what is taken from the abdomen by tapping in a dropsy; and this difference seems to be owing to the exhaling arteries of the brain being much smaller than those of the other parts.

The DIAGNOSTIC SIGNS of a Dropsy within the Brain.

HAVING given an account of all the various symptoms commonly attending a collection of water in the brain, I shall now recapitulate such of them as are the surest signs by which we may distinguish this disorder from others which so much resemble it as sometimes to deceive an experienced physician: And this will be the more necessary, as the ancients were altogether ignorant of the disease, and as the few of the moderns who treat of it seem to have described it more from theory than observation.

WHILE most of the later writers have confounded the signs of a dropsy

dropfy in the ventricles of the brain with thofe of the *hydrocephalus externus*, a few have more reafonably affigned to this fpecies of dropfy fuch fymptoms as commonly attend a compreffion of the brain, but without giving fuch a diftinct account of the firft appearance and progrefs of this diforder as could enable a phyfician to diftinguifh it from others of the head, from worms, from a foulnefs in the ftomach and bowels, or from a flow fever ending in a *coma*.

I have already obferved, that in the firft ftage it is hard to difcover this internal *hydrocephalus*. But when we meet with a patient under fifteen or fixteen years of age, feized with a flow fever of no certain type, and irregular in its acceffions and remiffions; when in that fever the patients vomit once a day, or once in two or three days; when they fhun the light, and complain of a pain in the crown of their head, or over their eyes, after the fever has continued for fome time, or of a pain thereabouts, that in fome days does not abate like the headach in ordinary fevers: When thefe complaints neither yield much to repeated vomits, gentle purges, nor blifters, I fay there is reafon to fufpect water in the ventricles of the brain. But as worms, and other diforders of the ftomach and intefines, are fometimes attended with moft of thefe, as well as other fymptoms that accompany the internal *hydrocephalus* in its firft ftage, we are often at a lofe to find out this difeafe, till it arrives at its fecond period, when the pulse begins to grow nearly as flow, or even flower than natural, but irregular; for this change of the pulse, added to the fymptoms of the firft ftage, is, as I have obferved, almoft an infallible fign of water in the brain, if at the fame time the patient is not relieved, and if the feverifh heat does not abate with the quicknefs of the pulse *.

WHEN the glands of the mefentery become fcirrhouf, the patients are liable to a flow fever; their pulse is quick and fometimes irregular, but is never fo flow as in health. In the cafe of worms
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* If we are to judge of the heat of the body in this difeafe, by feeling the hands and wrifts, we fhall be often deceived; for when thefe are expofed to the air, they become rather cold, while fuch parts as are well covered have a feverifh heat.

in the stomach and intestines, altho' the pulse be generally quick, yet sometimes it is slower than natural, and irregular; but when this happens, the skin is cool, and there is no fever. But in the dropsy of the brain, when the pulse becomes slow and irregular, neither the heat of the skin, nor any other of the feverish symptoms are sensibly abated: For in this case the motion of the heart is not accelerated in proportion to the degree of heat and fever.

WE often find a slow irregular pulse, in persons of a delicate habit, when labouring under cramps of the stomach, spasmodic colics, and violent nervous headaches, (as they are commonly called); but it is observable, that in such cases this kind of pulse is always attended with a cool skin.

WHEN therefore, with a slow and irregular pulse we meet with thirst and a feverish heat, watching, a *strabismus*, or double sight, a *delirium*, and screaming, succeeding the symptoms mentioned in the first stage, we may strongly suspect water in the ventricles of the brain. But this is still more evident, when soon after the patient grows comatose, the pupil dilates and loses its motion, the pulse becomes quick, the cheeks are flushed, the tendons start, and convulsions follow.

IT is true indeed, that some of these very symptoms are observed towards the end of common fevers, in which, from the brain being much affected, the patient falls into a *coma* before his death. But a fever from water in the brain is easily distinguished from others, by attending to the whole course of the disease, and particularly to the pulse, which, after having been at first quick, becomes slow and irregular; and lastly acquires a greater frequency than ever. Besides, the screaming, squinting, and dilatation of the pupil rarely occur in other fevers.

THE symptoms of no distemper resemble these of water in the brain so much as those which arise from worms in the stomach; for with a slow fever there is a want of appetite, vomiting, pain in the head, raving, and convulsions; but when worms in the stomach or

intestines occasion a flow and irregular pulse, the patients have not that feverish heat so observable in the internal *hydrocephalus*.

Of the CAUSES of a DROPSY in the Ventricles of the Brain.

THE immediate cause of this disease, and indeed of every kind of dropsy, is always the same, *viz.* such a state of the parts as makes the exhalant arteries throw out a greater quantity of fluids than the absorbent veins can take up.

THIS may be owing to several causes :

1. THERE may be an original laxity or weakness in the brain, whereby the small exhalant arteries of the ventricles will throw out the lymph faster than the absorbent veins can imbibe it.

IN children under a year old, I have frequently met with a *hydrocele*, or collection of water between the *tunica vaginalis* and the testicle, from such a cause : And this disease I have cured by small doses of rhubarb, by applying linen cloths dipt in brandy, or impregnated with the fumes of *myrrh*, *olibanum*, and *succinum*, to the *scrotum*, and by supporting the testicles with a bandage or truss. If in young children we could discover the dropsy of the brain as early as we do that of the testicles, and could apply our remedies as near to the part, we should probably often succeed in the cure : Tho' a dropsy in the brain would always be more unfavourable, as the circulation there is slower and more languid than in any other part.

2. ALTHO' there has been no original weakness in the brain, yet it may have suffered so much in the time of birth, by the compression of the skull, as afterwards to give rise to a collection of water in its cavities.

3. A scirrhus tumour of the *glandula pituitaria*, or in any part contiguous to the ventricles of the brain, by compressing the neighbouring

bouring trunks of the absorbent veins, will prevent the due absorption of that fluid which the small arteries constantly exhale, and occasion a dropsy in the brain; in like manner as a scirrhus liver, spleen, or *pancreas*, are often the cause of an *ascites*. As a proof of this, we may observe, that M. Petit often found the *glandula pituitaria* scirrhus in those who died of a dropsy of the ventricles of the brain.

IN one case I met with a hard tumour within the right *thalamus nervorum opticum*: It was almost as large as a small hen's egg, of a yellowish colour within, and of a firm consistence.

4. ALTHO' there may be no obstruction in any part of the brain, a dropsy may be formed in it, merely from a too thin or watery state of the blood. When the blood is too thin, the exhalent arteries will pour forth their fluids in greater quantity than usual; while the bibulous veins will absorb them more sparingly; and from this cause the water will be apt to accumulate, either in the *abdomen*, *thorax*, or brain, according as one or other of these parts is the weakest. I have known an instance of a dropsy in the cavity of the *abdomen*, where there were no obstructed *viscera* to be seen after death, and where the cause of the disease seemed to be no other than a dissolved state of the blood joined to an uncommon relaxation of the vessels.

ABOUT fifteen years ago, I had a patient who died of the *hydrocephalus*, probably owing to this cause; for this child, about a year before his death, and after the measles, falling into a bad state of health, the blood taken from his arm was observed to be preternaturally thin. From this time he never recovered his looks or strength; and, about ten months after, the symptoms of the *hydrocephalus* appeared. In this case I thought it probable, that the water began to be collected in the brain soon after the measles, which first broke the health of the child, and then the blood became too watery.

5. A suppression, or a diminished secretion of urine, may also
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give rise to this disease. Thus grown people, who die of an *ischuria*, have often water in the ventricles of the brain, and become comatose before their death; but such patients generally die before any considerable quantity of water is collected in these cavities.

6. LASTLY, in tedious chronic diseases, water is often collected in the ventricles of the brain, as well as in the cavity of the *pericardium*, but not in such quantity as to occasion the symptoms of a dropfy within the brain.

AN ATTEMPT to account for some of the most remarkable SYMPTOMS attending a Dropfy in the Brain.

IN general, the whole symptoms of this disease proceed from different degrees of the same cause, *viz.* the pressure or distension of the parts of the brain, occasioned by the water contained in its ventricles.

1. *THE loss of appetite and inclination to vomit*, are owing to the disordered state of the brain, between which and the stomach there is so great a sympathy, that in wounds of the head, where the brain is hurt, a vomiting is almost a constant symptom.

2. *THE aversion to light, in the first and second stage of the disease*, proceeds from an increased sensibility of the *retina*; and this is probably owing to the irritation of the *thalami nervorum optitorum*, in consequence of the water accumulated in the anterior ventricles of the brain.

3. *THE slow irregular pulse in the second stage.*

THE motion of the heart is owing to the irritation of the returning venous blood poured into its ventricles. This irritation, however, could have no effect upon the heart, were it not for its sensibility,

ty, which depends intirely on its nerves. Wherefore, in a *hydrocephalus*, when the water is collected within the brain in such quantity as to press, with a considerable force, on the medullary substance, the nerves proceeding from it will in some degree lose their powers, and consequently the heart will be less sensible. And hence the pulse becomes often as slow, and sometimes slower than in a natural state, altho' there be a real fever in the body; which fever, were it not for this pressure on the origin of the nerves, would occasion a quick pulse.

WHEN, in this disease the pulse is slow, it is always more or less irregular; and this may also be owing to the nerves of the heart being, in some measure, deprived of their usual power, by which means that organ cannot move with its wonted steadiness and regularity.

4. *THE quick pulse in the third stage.*

OF all the symptoms that attend a dropsy in the brain, there is none so hard to be accounted for as the quick pulse towards the end. For if the pressure of the water occasioned the slow pulse in the second stage, one would imagine that in the third, when this pressure is increased, the sensibility of the heart should be still more impaired; and that therefore its motion should be slower, instead of being quicker. However, we find in fact, that the pulse is remarkably quicker towards the end, when the pressure of the water must be greatest; let us therefore inquire what may probably be the reason of this symptom.

WHEN, in the second stage, the pressure on the sides of the ventricles of the brain occasions the slow irregular pulse, it seems to produce this effect, by lessening the sensibility and other powers of the cardiac nerves. When in the third stage the water increases, this pressure must be greater; and therefore it might be natural to think, that these nerves should be rendered still more unfit for performing their function. But we must consider, that when the sides of the ventricles are stretched by the water beyond a certain pitch, the violence

lence done to the medullary fibres of the brain causes such an uncommon irritation as must quicken the pulse: For in animals newly dead (where we must suppose the nerves to be still more insensible and unfit for action, than in the third stage of the *hydrocephalus*) an irritation of the *medulla oblongata* restores the motion of the heart; and if, as I have observed above, the volatile salts held to the nose, or cinnamon-water taken into the mouth, by their *stimulus*, though for a short time, give new vigour to the nerves of the *uvea*, (which towards the end of this disease begin to lose their powers), why may not the irritation of the medullary part of the brain, occasioned by the immoderate distension of its ventricles, so affect the nerves of the heart as to accelerate its motion?

IN an apoplexy, the pulse, tho' at first slow, becomes very quick towards the end; and indeed, in almost every disease, the pulse is uncommonly quick before death, not because the nerves of the heart are then more sensible, or fitter for performing their office, than they were before, but because at that time there is an uncommon struggle in the body, and all its powers are excited into action by the great irritation of the brain and nervous system. The same seems to be the case in those who are dying of a dropsy in the brain; for how much soever the medullary part of the brain may be compressed, yet the convulsions which happen in the last stage show that the brain and nerves are sensible of irritation, and still retain their power of putting the muscles in motion.

5. *THE dilatation of the pupil.*

THE contraction of the pupil is owing to the uneasy sensation excited in the *retina* by too much light; and hence it is, that in a dark place, or when the *retina* becomes insensible of the *stimulus* of light, the pupil is always observed to be wide. In the *hydrocephalus*, when the water in the ventricles presses so much on the *thalami nervorum optidorum* as to render the optic nerves in a great measure insensible,

sensible, the *retina* will no longer feel the impression of light ; and therefore the pupil will remain dilated.

IN the account of the symptoms of the third stage, I mentioned an instance of a boy of five years of age, whose pupils were much dilated on the fifth day before he died ; but we observed them next day to be as much contracted as is usual in a person in health placed in a moderate light. At this time, having endeavoured to rouse the patient, by holding a volatile spirit to his nose, and making him swallow some cinnamon-water, the pupil instantly became as wide as it had been the night before. In about half an hour after, I found the pupils again contracted ; but they were presently enlarged as before, upon holding the spirit of *sal. ammoniacus* to his nose. This experiment I repeated four times in two days, and always with the same success.

IN this case the dilatation of the pupil was at first owing to the compression of the *thalami nervorum opticorum* by the water contained in the anterior ventricles of the brain. But soon after, the origin of those nerves which serve the *uvea* being also considerably compressed by the increased quantity of water, the longitudinal fibres of this membrane (which by their natural contractility dilate the pupil) become paralytic and flaccid, as happens in the bodies some time after death ; wherefore the edges of the pupil being less drawn outward, of course it would become smaller.

THE volatile spirits applied to the nose, by irritating its nerves, so affected the brain as to give some vigour for a short time to the nerves of the *uvea*, by which means its longitudinal fibres, regaining their power of contraction, immediately dilated the pupil ; but as soon as the effect of this *stimulus* ceased, the fibres of the *uvea* being again deprived of their contractility, the pupil returned to its former dimensions.

6. *THE slow respiration towards the end of the disease.*

IN this kind of breathing (which I have also observed in patients who died of an apoplexy and an *ischuria*) there is a considerable

considerable pause after every expiration before a new inspiration succeeds. This pause is ordinarily for a few seconds; but I have sometimes observed it longer, and in one apoplectic case it continued above half a minute. Now the brain being greatly compressed, the uneasy sensation arising from the difficulty the blood finds in passing through the lungs will be much less felt than usual: Hence, after expiration (which is performed by the power the cartilages of the ribs have to restore themselves) a long pause intervenes before a new inspiration takes place; because the mind is not excited to put in motion the muscles concerned in inspiration, till the sense of suffocation in the breast becomes so great as to rouse, as it were, the sentient principle from its lethargic state.

Of the CURE of a DROPSY in the BRAIN.

IF this disease could be known early, and before any considerable quantity of water has been collected, it might probably be sometimes cured by purgatives, diuretics, blisters, frictions, exercise, and diet. But as it never discovers itself till so much water is accumulated as, by its pressure on the sides of the ventricles, to disturb the action of the brain, we have little to hope from any medicine. An *ascites* indeed has been often cured by diuretics, or purgatives. But if we consider the distance between the brain and the *abdomen*, (where these medicines by their *stimulus* increase, in a particular manner, the action of the absorbents, at the same time that they evacuate the watery part of the blood), the extremely slow motion of the fluids in the small vessels of the brain, and the pressure of the water on the sides of its ventricles, which must render the absorption of that fluid still more difficult, we shall see the reason why diuretics and cathartics should be so inefficacious here.

IN an *ascites* the patient is generally relieved, and sometimes cured by tapping; but in a dropsy of the ventricles of the brain, any

ny such attempt to draw off the water could have no other effect than to hasten death.

I freely own, that I have never been so lucky as to cure one patient who had those symptoms which with certainty denote this disease * ; and I suspect that those who imagine they have been more successful have mistaken another distemper for this. I remember several years ago, that an able and experienced physician being called to a child of a year old, in a fever attended with convulsions and a *coma*, was of opinion, that the disorder proceeded from water in the head ; on which account, besides blisters, which had been applied before, he ordered a purge of jalap and calomel, which had a very good effect ; for in two or three days the *coma* and convulsions ceased, and the patient soon recovered ; which, I am persuaded, could not have been the case, had he laboured under a drop-sy of the brain. Farther, this child was not only suddenly seized with the fever, (as commonly happens when it takes to the head), but at no time of his illness had he either an irregular or a slow pulse, or indeed any number of the other symptoms which I consider as essential for distinguishing the *hydrocephalus internus* from another disease.

THE END.

* The medicines I chiefly used were repeated purges of rhubarb or jalap, with calomel and blisters ; by which last I have seen the patients somewhat relieved for a short time in the second stage. I have also ordered the powder of *asarum* to be drawn up into the nostrils, with a view to make a discharge of a watery humour from the vessels of the head.

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A P P E N D I X *.

An ACCOUNT of an Epidemic Distemper at Edinburgh, and several other parts in the South of Scotland, in the Autumn of 1758; in a Letter to Dr John Pringle †.

Edinburgh Nov. 10. 1758.

THE month of May, this year, was remarkably dry and hot. June was cold and dry. In July and August, we had but just as much rain as was sufficient to bring forward the fruits of the earth: The air was temperate, or perhaps a little warmer than is common in Scotland during those months. Towards the end of August, and for the first week of September, the weather was warmer than usual in that season; but less so than at the same time in the preceeding year. From the 8th to the 16th of September it was mild. From the 16th to the 20th, we had a strong easterly wind, which, though not extremely cold, yet cooled the air considerably. During the whole month of September, and till the 8th of October, we had scarce a shower sufficient to lay the dust. On the 8th of that month, we had a violent wind from the north-east with rain, which continued thirty hours. From the 8th to the 26th, the weather was mostly clear and frosty, with some gentle breezes. From the 28th of October to the 8th of this month, the winds have been southerly, and accompanied with wet.

DURING the months of July, August, September, and October, the wind blew more from the east than ever had been known be-

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* The two following papers having been omitted in making the collection, they are here inserted by way of appendix. And as the index was printed before these papers, it is hoped, that the reader will excuse his not finding in it any references to them.

† First published in vol. 2. of the London Medical Observations and Inquiries, in the year 1762.

fore in this country, at that time of the year. In August and September, we had seldom any strong winds from the west as usual; from whatever point they came, they were moderate; and although it was calm weather for many days together, yet, as far as I can recollect, there was not one foggy day during the autumn. The barometer was higher throughout September, and the greatest part of October, than ordinary.

I thought it proper to lay before you this account of the weather, in order to judge how far any of the sensible changes of the air might influence the health of the people here. But for my part, considering how remarkably mild and dry our season was, I can hardly ascribe the rise of our epidemic to any of the known qualities of the air.

BEFORE I proceed to the description of that distemper, it may be proper to take notice, that during the months of July and August, a fever, with a bloody flux, raged in Lorn, and other parts of Argyleshire; and was not only mortal among the common people, but carried off several persons of a higher rank. The same disease prevailed no less at Newcastle upon Tyne, in August and September; and likewise at Haddington*, about the same time, but in a less degree. In the months of September and October, we had a bad sort of small-pox at Edinburgh, and in other parts of this country. In some parishes near Coupar in Fife, eight died out of twenty-eight; and in some parts of Teviotdale, three or four died for one that recovered.

As for what I call the epidemic, it was first taken notice of in this city, soon after the change of the weather upon the easterly winds that blew from the 16th to the 20th of September: Several children began then to be affected with a slight degree of fever, attended with the common symptoms of a cold; but this was not thought extraordinary at that season of the year. About the end of September, the distemper grew much more general, both here and in the neighbourhood; and in the last week of that month,
in

* A town within twelve computed miles of Edinburgh.

in the space of two or three days, thirty boys out of sixty, at the grammar-school of Dalkeith *, were seized with it. In the beginning of October, the sickness became still more frequent at Edinburgh, Dalkeith, and throughout a great part of the Lothians †. Old as well as young were taken ill: Nay, even women in child-bed, who were not exposed to the cold air, were affected; and in particular, I knew one, who had but just recovered of a dangerous fever, after her delivery.

THE sickness continued to increase in all the places above mentioned, till about the 24th of October, when it began to abate: But whether the decline was owing to any alteration in the air, or because the distemper had already seized most people, I cannot determine; though the latter seems more probable, as I am persuaded, that in Edinburgh, and the neighbourhood, not one out of six or seven escaped; and I am assured, that in some places it was still more general.

HITHERTO I have only mentioned the rise and progress of this epidemic here and in the places adjacent. I shall now, as far as I have been able to learn, inform you of the time of its appearance in other parts of the country. In Fife, about Kirkaldy, it was not observed till the first week of October; at St Andrew's, not till the 10th or 12th of that month. In Angus, it began sooner. In Perthshire, it raged most between the middle and end of October; and many died of it. In the shire of Air, and at Glasgow, it was at the worst after the middle of October. In Teviotdale, it began later. At Ormiston, a village only four miles from Dalkeith, it did not appear before the 15th of October. And at Whitburn, about half way between Edinburgh and Glasgow, it was little taken notice of till towards the end of that month. I have been informed, that the same kind of illness prevailed thro' Aberdeenshire, and other parts in the North; but that, at the end of October, it had not reached the shire of Ross. A gentleman

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told

* A village four miles from Edinburgh.

† The shire of Edinburgh, and the two adjacent shires, so called.

told me, that in the Carse of Gowrie *, in the month of September, before this disease was perceived, the horses were observed to be more than usually affected with a cold and a cough.

HAVING given you this summary account of the epidemic, I shall now more particularly describe the symptoms. In general, people were differently affected. Some complained first of a slight sore throat, with a feverishness; and, after a few days, they were seized with a cough. Many had a heavy dull pain in their forehead, with watery eyes; either a sneezing or discharge at the nose; but with little fever. Some, all at once, felt a soreness in the inside of the *trachea*, as if that part had been excoriated. Others had a hard dry cough, without this soreness; but generally with a quick pulse. Several were attacked with a slight *diarrhœa*; and others with bleedings at the nose, sometimes profuse, and continuing for several days; till either by the hæmorrhage, or by opening a vein, the pulse returned to its natural state: For in all those who had this symptom, the pulse was not only quick, but, for the most part, remarkably full. Two of my patients were troubled with a severe pain over their whole head, but had little or no fever. In one, the head-ach becoming periodical, went off upon lying a bed, encouraging perspiration, and taking an electuary of the bark, with some glasses of claret: This person having weak nerves, could not bear evacuations. The other had his whole head blistered; leeches applied to the temples; took camphire, *tinctura sacra*, and *laudanum*; but with little benefit. Sudorific boluses of *gum. guaiac.* with *sal ammoniac. volat.* seemed to do him most service. After taking a few doses of this medicine, the pain left his head, and seized the loins and right thigh, but more slightly, and then went off gradually; from whence it appears, that those pains were of the rheumatic kind. Some complained of pains only in the cheek-bones, teeth, and sides of their head. Others had a fever without a headach, sore throat, or cough, or indeed any other symptom, in the beginning: But when the fever began to abate, as it usually did in

* The name of a large vale, on the north side of the river Tay, in Perthshire.

in a few days if the patient lay a bed, more or less of a cough succeeded. In two patients, the cough seemed to be critical; for it no sooner took place, than the pulse returned almost to its natural state. One of them, a married lady, aged above thirty, had been feverish for four days; a rash or scarlet eruption appeared, but did not come fully out; and as she was restless and uneasy, I was sent for about ten at night. Her pulse then beat 120 times in a minute, and was full; but as she had a moisture on her skin, I delayed ordering her any medicine, till I should see her again in the morning. About midnight she was seized with a troublesome tickling cough, which hindered her from sleeping. At half an hour past four, being called up to see her, I found her skin cooler, her pulse less full, and beating only 96 times in a minute. After this she slept; and at eight in the morning I found her pulse down at eighty. Here it should seem that the morbid matter, not thrown off by the skin, had fallen upon the *trachea*; so that the cough might be said to have been truly critical. Few, upon being taken ill, complained of any coldness or shivering, commonly the first feverish symptoms: However there were some who were seized in a more violent manner, and with the *horror febrilis*; especially when, from want of care, they had a relapse, which was often much more severe than the first attack.

IN regard to your question, Whether the distemper was infectious or not? As far as I have observed myself, or been informed by others, our epidemic did not spread by contagion, from one person to another, like the plague, small-pox, or measles; but seemed to be owing to some particular quality in the air. Those who attended the sick were not more liable to be affected than others; and I myself escaped, notwithstanding my visiting many of the sick, and being obliged to travel frequently into the country, and sometimes in the night. I had no opportunity to observe, whether nurses infected the children they suckled, or the infants their nurses; but I do not believe they did, from what I remarked in other cases.

As to the cure. In the beginning, when the disease was mildest, it generally yielded to lying in bed, keeping the body open with

with clysters, and promoting sweat by warm diluent liquors. Afterwards, a higher degree of fever, which many had, required bleeding; and then the blood was almost always fizy, even in those who had no fixed pain, nor any considerable symptom, except heat, and a quick pulse. In some, especially among the country-people, the coat of the blood, instead of being tough, thick, and fizy, was transparent like a jelly; the *crassamentum* was of a loose texture, and separated but little *serum*. For the cough, and foreness of the *trachea*, the usual medicines were ordered: But when the patients only complained of a dry tickling cough, attended with little or no fever, a dose of *laudanum* at bed-time was the best remedy.

THOSE who exposed themselves too soon to the cold, before they had perfectly recovered, frequently relapsed; were often worse than at the first attack; and generally required more bleeding. Many were so slightly affected, as to need no medicine at all.

FEW died of the disease, especially when it first appeared, except some old people; or those whose lungs had been greatly obstructed, or long oppressed with phlegm. In such cases, besides bleeding, when the pulse required it, blisters and boluses of *gum. ammoniac.* with *sal. ammoniac. vol.* were the chief remedies I used, with a slight pectoral decoction, or infusion, acidulated with vinegar.

TOWARDS the end of October, and in the beginning of November, the distemper, especially if neglected at first, became more dangerous, by falling on the lungs, *pleura*, or muscles of the *thorax*. Then repeated bleedings were requisite; and afterwards, blisters to the parts affected had remarkable good success.

IN some parts of the country, when the disease was not taken care of in the beginning, as being attended with no alarming symptoms, it assumed the form of a slow fever, which sometimes proved mortal.

EXTRACTS of several LETTERS to Dr PRINGLE, relating
to the use of the SUBLIMATE, in the cure of Phagedænic Ul-
cers *.

Extract of a Letter, dated Edinburgh, January 15. 1757.

WE have had several instances here, of carcinomatous or phagedænic ulcers of the face, cured by the Baron Van Swieten's medicine for the *lues venerea*; I mean the solution of the corrosive sublimate in malt-spirits †. We give it from one to two table-spoonfuls a-day; and also direct the sores to be washed with it. In one instance, by the internal use only, it cured a person whose whole face was over-run with a sore of this kind: But the cure was not performed till after three months; during which time the patient took between two and three quarts of that medicine.

Extract of a Letter, dated Edinburgh, March 17. 1757.

YOU having observed, in a former letter, that the word *phagedænic* was of a very vague signification, I have inclosed the cases of two patients, taken from the register of the Royal Infirmary: The one, of a carcinomatous ulcer on the cheek and nose; the other, of an ulcer in the leg, of that kind commonly called scorbutic, which were both cured by the solution. We had another instance of the efficacy of that medicine still more remarkable. A woman
of

* First published in vol. 2. of the London Medical Observations and Inquiries, in the year 1762.

† Dr. Whytt always supposes this medicine to be made according to the proportions mentioned in the first volume of the London Medical Observations and Inquiries,

of Dalkeith, about fourteen years ago, happened to scratch a scab or wart that grew on one of her temples; upon which, an inflammation or foreness of the part ensued, spreading over the whole face, eating away great part of the lips, and the point of the nose, and running down upon the skin of her neck to the clavicle. This woman, by using three or four quarts of that mercurial, was, in the space of three or four months, to appearance, cured; though the skin of her face still looked as parts commonly do after having been severely burnt*.

I shall only trouble you at present with another case, of a man about 57, who was a patient of my own, and had a kind of cancerous ulcer on his nose, near the inner angle of the eye. He had spit three weeks with the *pil. mercurial. Pharm. Edinb.* during which time the sore became manifestly worse. I made him wash the part frequently every day, at first with the common solution, and afterwards with a stronger preparation of the same kind, *viz. scrup. i.* of the sublimate to a pint of spirits. At the end of three or four weeks, the greatest part of the sore looked better, had less of a granulated substance, and seemed to be growing well; but in other parts it appeared rather to spread. As he was going to the country, I gave him a quart of the medicine for internal use, and a stronger solution for external application: But since that time I have heard nothing of him. The sore which this man had, was of the kind called *herpes exedens*, *nome, noli me tangere*, or *ulcus depascens*†.

IT

* Dr. Whytt gives the sequel of this case, in a letter dated the 11th of November 1758, in these words: "The woman of Dalkeith, whose head, face, and breast were over-run with a *herpes exedens*, or phagedænic sore, was to appearance quite cured by the sublimate; yet after discontinuing it for some considerable time, the sore began to break out again, but was soon checked, by returning to the use of the solution; since which, she has been frequently threatened with a relapse, but has always been able to prevent it by having recourse to her medicine."

† Dr. Whytt, in a letter dated the 30th of April 1757, informed Dr. Pringle, "That the same patient having been with him two days before, he had been surprised to see such a change on the sore for the better. That the rugged malignant appearance was quite gone;

It would seem from this last case, compared with the others, that the solution has greater effects in curing these malignant ulcerations, when taken inwardly, than when used only as a topical medicine: From which one would be apt to conclude, that such sores do not intirely depend on a morbid state of the part affected, but also on some fault in the blood, which being corrected by the sublimate, the ulcers are soon healed.

The two cases referred to by Dr WHYTT, taken from the register of the Royal Infirmary.

CASE I.

EDINBURGH, Nov. 21. 1757. WILLIAM KERMOCH, aged 28 years, had several ulcers of a carcinomatous nature on his cheek, nose, and upper lip. The ulcer on the upper lip had eat quite through, and the parts all about it were hard, and considerably swelled. That on the cheek, run up on the side of the nose, very near as far as the internal *canthus* of the eye; a spreading inflammation, and thick hard scabs, were observable all around the ulcers; and they discharged a thick whitish matter in small quantity,

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tity,

“ gone; and that instead of *ichor*, the sore afforded a thick, white, and well concocted matter; had the colour of the most benign ulcer; and was contracted to less than half its former dimensions. That the patient having returned to the internal use of the solution, had taken another quart, at the rate of a spoonful morning and evening. That the medicine did not bring on a spitting, nor was attended with any other inconvenience. That, at the same time, he continued to wash the sore, twice a day, with the strong solution: And that, in order to obtain a complete cure, the patient was directed to continue the medicine for six weeks, or two months longer, if the part was not healed before that time.”

Dr. Whytt added, in a subsequent letter, “ The man who had the cancerous sore, or *noli me tangere*, on the *os unguis* of the right side of the nose, grew much better by the internal use of the sublimate; but as he lived at too great a distance from Edinburgh, he could not be supplied with a fresh quantity, when he had exhausted that which he carried with him. However, by washing the sore daily with the stronger solution, he had continued a twelvemonth without growing worse.”

tity, but of an offensive smell. He said they were occasioned by his falling under some heavy load, by which he hurt his face.

AT first, an emollient cataplasim was applied to his cheek; and he took the *decoct. tamarind. cum trip. fenn.* for a purge. Then he was put into a course of the solution of sublimate, taking one spoonful morning and evening, with the *decoct. lignor. pharm. Edinb. lib. ii.* daily. For the first three days, these medicines griped him, and occasioned a pain in his stomach; upon which they were intermitted, and he took, at bed-time, a bolus of twenty-five grains of rhubarb, with sixteen drops of *laudan.*; which removing the gripes, he went on with the solution for about three weeks, when a swelling was observed on his right nostril, and upper lip. His medicine was again intermitted, and he took a bolus of jalap with calomel. The swelling going off in three days, the solution was renewed, and continued for about twenty-six days longer. By this time, the ulcer was almost intirely healed; but some hardness and swelling still remaining upon the lip, the medicine was again interrupted, and, for six weeks, the patient was ordered the *pill. mercurial. laxant. pharm. Edinb.* in the dose of *drach. ss.* every other night; after which, he seemed to be perfectly cured: For the ulcer skinned over, and nothing but a little hardness remained upon the side of the lip. He had no spitting during the long use of these mercurials.

C A S E II.

EDINBURGH, September 12. 1756. Peter Morison, aged 56 years, about five years before his admission, had a *cachectic** ulcer upon the inner angle of the left leg, which was apparently cured, but broke out again a few weeks after, and was never thoroughly healed; tho', when

* By a *cachectic* ulcer, is meant, one of those ill-conditioned sores with livid edges, commonly, but improperly, called scorbutic.

when he came into the infirmary, the fore was contracted, superficial, and scarce discharged any matter. The whole leg was considerably swelled, especially towards night; the parts around were hard and itchy, but never painful. He had also some asthmatic complaints.

AFTER the patient was admitted, and was using some medicines for his asthma, a common caustic was applied to the fore. When the eschar was thrown off, an ulcer appeared, about four inches in length, and two in breadth, with the edges blue and callous. By the constant use of fomentations, the swelling of the leg was considerably diminished; but though he took mercurial purges, had the edges of the ulcer frequently scarified, dressed with *ung. arcei*, and washed with the tincture of myrrh, yet it continued in much the same condition. He took vomits of *ipécacuanha*, squill-mixtures, tar-water, and was blistered for his asthmatic disorder; but without success.

ABOUT the 12th of February 1757, he began to take Van Swieten's solution of sublimate, to *unc. ℥ss.* morning and evening, which agreed very well with him, made him sweat plentifully, and considerably increased the quantity of his urine. By thirty days use of the medicine, the fore healed up, though the skin remained tender; but the cough, difficulty in breathing, and pain in the breast continued.

Extract of a letter, dated Edinburgh, Nov. 10. 1757.

THE solution of corrosive sublimate lately dissolved, in a short time, a glandular knot, which rose on the under part of the lower jaw, after cutting off a cancerous lip. Mr George Cleghorn of Dublin writes to me, that this medicine has been very successful in venereal cases; but mentions an observation which is new to me, *viz.* that in patients who are under no confinement, and walk abroad, the mouth is seldom affected; and the evacuation

by the skin and kidneys is much less than in those who keep the house. He adds however, that the former are longer in being cured than the latter.

Extract of a letter, dated Edinburgh, January 27. 1759.

SINCE I wrote to you last, I received the inclosed account of the effects of the sublimate in the case of MARGARET BRUCE, whom I saw at Cramond in November last; at which time I examined all the parts that had been once sore, but were then perfectly healed by the use of that medicine, given her by Mr Spotiswood, surgeon of that place. Although that gentleman's account of the cure would have been satisfactory enough, yet I had the truth of it confirmed by the Reverend Mr Gilbert Hamilton, minister of the parish, who, after reading the case drawn up by Mr Spotiswood, delivered it to me. As this is one of the strongest instances of the efficacy of the sublimate, in curing those obstinate ulcers of the phagedænic kind, I thought it would be agreeable to you, to have the whole account as it was delivered to me, with Mr Spotiswood's letter on that occasion.

Copy of Mr SPOTISWOOD's letter to Dr WHYTT, dated Cramond, December 9. 1758.

S I R,

IN compliance with your request, I have sent you, inclosed, an account of the effects of the sublimate in the case of Margaret Bruce, in which they were more remarkable than in any that I have yet seen.

I shall take this opportunity to acquaint you, that I have another patient, who, since the 17th of August last, has used fifty-six grains of that medicine. Before he began, he had a very fordid ulcer in his nose, by which the whole *septum narium* was eat away; the
nose

nose was swelled and painful, with a redness and inflammation externally. He had not taken seven grains, before he was eased of the pain, and the sore had a better appearance. I washed the parts with *aq. calcis* and *mel. rosar.* and dressed the ulcer with dry lint. By continuing this course, the discharge was lessened, a stop was put to the corrosion, and the ulcer was cleansed: But the external parts inflamed and suppurated, first on one side of the nose, then on the other; and the two ulcers joining, most of the nasal bones came away. How this case may end, I cannot foresee; but this I think remarkable, whenever the sublimite is intermitted, the parts become more painful, discharge a greater quantity, and produce some bad *fungi*; but upon repeating that medicine, the appearances soon grow better. This man, by trade a miller, has scarce lost an hour's work, or one meal, during the use of the mercurial. He took it when at harvest-work, and is still taking it, notwithstanding the coldness of the season. He complains of being qualmish after each dose. It acts as a laxative; but as for other evacuations, either by sweat, urine, or *saliva*, they are scarce more than natural. He has a wife and several children all healthy; and I can see no reason to suspect any venereal taint.

I am, SIR, &c.

R. SPOTISWOOD.

The Case of MARGARET BRUCE referred to in the above Letter.

MARGARET BRUCE, a young woman of this parish, of mean condition, enjoyed a good state of health, till the 18th year of her age, when she was seized with convulsions of the epileptic kind, and other ailments. In January 1755, when she was twenty-two, she complained of violent pains in her right leg, attended with a hardness and swelling, but without any appearance of matter, or inflam-

inflammation. After several fruitless applications, I laid a blister along the *fibula*, which seemed to be the principal seat of the pain; but the blistered part, instead of healing, degenerated into a foul ulcer, which I could never bring to a good digestion; for it continually produced a whitish slough, which, if removed, was sure to return in two days.

IN the month of May following, my patient was sent to the Royal Infirmary at Edinburgh, where she staid five months; and, during that time, had the sore regularly dressed, and all means used to cure it. Among other attempts, the whole ulcerated part was destroyed by a caustic, and forty peas put into the cavity: She took many purges with calomel, the mercurial pills of the Edinburgh Dispensatory, and a decoction of the woods. But none of these medicines had any other effect, than to make her void several worms; so that she was dismissed about the middle of October not cured.

IN the end of February 1756, she was again sent to the Infirmary, where she continued upwards of three months, and was ordered some mercurial laxative pills, which affecting her mouth, she spit, for some time, three pounds a day. She had an issue put in the inside of the lame leg, which gradually widening, came at last to hold thirty peas. She was dismissed again in the beginning of June, in a better condition, but still far from being cured.

AFTER this, she used various other remedies, and, among others, sea-water, for a considerable time, but without any benefit. At last observing, that none of all these methods promised a cure, I luckily thought of the sublimate, as having had some experience of its good effects in similar cases. But, previous to the account of its success here, it may be proper to describe more particularly the state in which the patient was, when she began to use this medicine.

THE oldest sore, which broke out in January 1755, about three inches above the *malleolus externus*, upon the *fibula* of the right leg, was not broader than a crown-piece, but had large callous edges, and the muscular parts beyond them felt hard: This ulcer was
round

round and foul at the bottom, without any great discharge; the *fungus* which arose from it, though frequently cut away, was renewed in a few days.

THE issue, made at the ordinary place, on the inside of the same leg, by the number of peas, and length of time, had fallen much lower, and had degenerated into a sordid ulcer, of the shape of the figure 8, and with the same kind of indurations around it as in the other.

ABOUT May 1756, a glandular tumour, very painful, was formed at the upper part, and upon the inside of the thigh of the same side, which was sometimes larger and more uneasy than at other times.

IN October 1757, a sore broke out upon the breast of the same side: This was superficial, without any hardness about it, but always foul, and could not be healed.

IN June 1758, two new ulcers broke out upon the same leg, which, though small, were also always foul, and could not be cicatrized.

AROUND these sores, and indeed almost all over the leg, the skin was of a blackish colour, with scurfs and scales, which soap and water could not remove. The whole leg was swelled, and felt hard; the patient complained of pains striking through it; she could not extend it, (though the tendons were not contracted), but went on crutches with the limb suspended, since May 1756. Notwithstanding all these ailments, the want of exercise, and her low circumstances, (for she was maintained by the parish), the patient kept up her flesh, and was otherwise in better health than could have been expected.

UPON the 14th of September last, I began the sublimate in this manner. I dissolved seven grains in eight ounces of spring-water, and gave morning and evening a spoonful of this solution. The whole was taken in eight days. After three days intermission, the same quantity was renewed, and continued for seven days longer, when the *menfes* coming at the usual time, I forbore giving her the remaining ounce, till she was in a proper condition to take it; and, on that account, she was five days without any medicine. She vomited.

vomited after the two first doses; and all the rest occasioned a *nausea*, and sickness, with a burning heat from her stomach to her throat, but no vomiting. She had few stools, till the third day after taking the solution; but, from that time, she had more or less of a purging; some days, six or seven motions, on others, not half that number. She sometimes complained of sharp gripings, but more generally of a rumbling in the bowels. After the second dose, a copious sweat broke out, particularly on the fore leg, which was never known to perspire before. From the fourth day after she began the solution, she spit about three or four pounds a day; but although she complained of a pain in her teeth, her tongue, and *fauces*, yet the swelling of those parts was much less than what is observed in a salivation raised by calomel, and her breath was much less offensive. In five days after she began the course, she made twice her usual quantity of water, which was of a deep colour, and deposited a large sediment.

ALL these evacuations, *viz.* the stools, the profuse sweats, the increased quantity of urine, and the spitting, not only continued while she was using the medicine, but for a fortnight after.

THE effects it had upon the ulcers were no less remarkable. In four days from her beginning, the two sores that had broke out last upon her leg, and that on her breast, were perfectly healed; the other two ulcers on the leg looked cleaner; the callous edges, and hardenings round them, were diminished; the blackness of the skin disappeared; the muscles felt softer; the glandular tumour was less; and the patient said, she had not been so easy and free from pain, since the first breaking out of her sores. In short, in eleven days from the use of the sublimate, the two remaining ulcers were perfectly healed, and all the dressings removed: After which, the skin, where the sores had been, cast off several different crusts or coats, and the cure was compleated.

THE patient was much weakened by her great evacuations; but she has now recovered a good degree of strength, the skin is whole, the leg without swelling, or pain, and she walks without any support.

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A.

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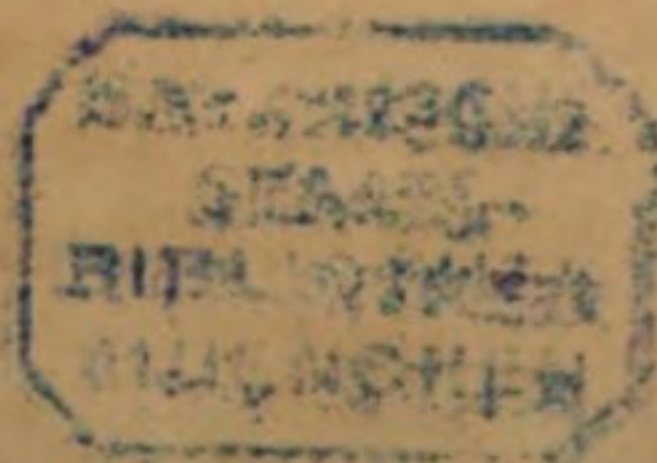
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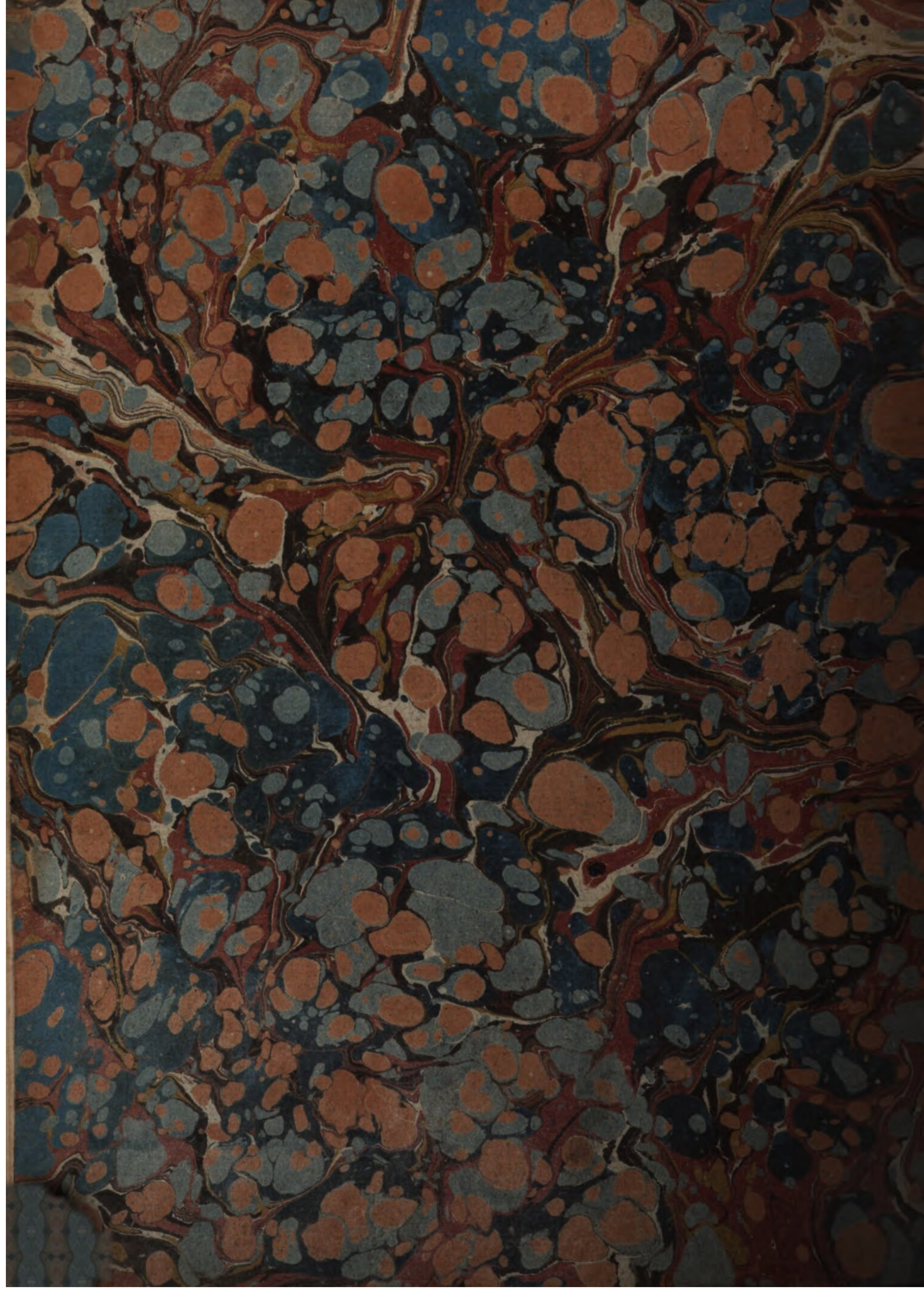
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Whytt, Robert,

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Edinburgh 1768

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